

VOLUME 81 • NUMBER 5 • DECEMBER 1976

# The American Historical Review

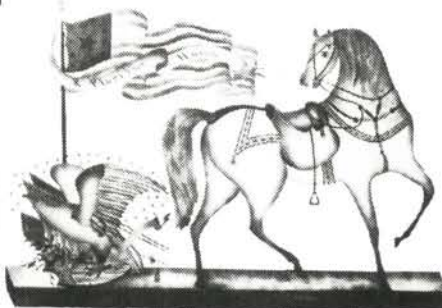
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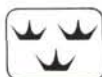
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*The American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003, and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228.

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## Science and Humanism in the Italian Renaissance

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ERIC COCHRANE

SOME FIVE DECADES AGO, in the heat of the revolt against the Burckhardtian view of the Renaissance, science and humanism were generally regarded as antithetical, or at least as completely unrelated historical phenomena. In chapter 66, Volume IV, of his monumental *History of Magic and Experimental Science*<sup>1</sup>—a chapter appropriately entitled “Humanism in Relation to Natural and Occult Science”—Lynn Thorndike noted that, except for occasional references to astrology, only a few of the well-known representatives of quattrocento humanism ever wrote anything on subjects that might be considered scientific. Of these, Pier Candido Decembrio gave “a derivative and amateurish performance” in the single one of his some 127 works that strayed from the narrow path of philology and rhetoric. Similarly, Galeotto Marzio merely paraphrased Peter of Albano in the *De Incognitis Vulgo* that he addressed to Lorenzo the Magnificent; and his “miscellaneous rambling” happily led him rapidly on to subjects in which he was more qualified—like the efficacy of prayer and the intercession of saints. Thorndike could not but conclude that “the fifteenth century strikes us as distinctly inferior to the fourteenth” in all of the sciences except perhaps surgery and anatomy; and he suggested as a possible cause for this decline the draining off of Italian talent into what he called “humanism and painting.”

Thorndike’s thesis was supported at the time by the contribution of the pioneer of modern studies in ancient science, George Sarton, to a symposium published just a few years earlier with the intentionally Burckhardtian title of *The Civilization of the Renaissance*; and it was corroborated some twenty years later by Sarton’s lectures on several Renaissance scientists.<sup>2</sup> Science, said Sarton, has been introduced into Western culture not once but twice: first in the twelfth century, with the translation of Arabic mathematic and scientific texts, and second in the seventeenth century, in the course of what became

A revised version of a paper originally presented at the seventh annual conference of the Center for Medieval and Renaissance Studies at the Ohio State University in February 1976.

<sup>1</sup> Lynn Thorndike, *History of Magic and Experimental Science* (New York, 1934).

<sup>2</sup> George Sarton, *The Civilization of the Renaissance* (Chicago, 1929); *Six Wings: Men of Science in the Renaissance* (Bloomington, 1957); and *The Appreciation of Ancient and Medieval Science during the Renaissance, 1450–1600* (Philadelphia, 1955).

known as the "Scientific Revolution." But whereas after its second appearance science succeeded in becoming a permanent part of Western culture, soon after its first appearance it was swept away by its antithesis, that is, by Italian humanism. For humanism turned the minds of prospective scientists from fact to form, from substance to grammar, from empirical investigation to the adoration of ancient authorities. True, Sarton admitted, a few humanists showed signs of appreciating at least scientific methodology: Leonardo, Pico, and Machiavelli. But Leonardo hid his writings from his contemporaries. Pico died prematurely. Machiavelli proved unable "to emancipate himself from the vanity and corruption of his environment." And all of them were thoroughly forgotten by those who were to reintroduce science in the age of Galileo and Descartes.

The views of Sarton and Thorndike fitted very well with views current at the time concerning other aspects of Renaissance culture. The political historian Ferdinand Schevill insisted that creativity in the realm of politics came to an end at the beginning of the fifteenth century. It was then, he said, that the "guild democracy" of the medieval communes finally succumbed to plutocracy and dictatorship, while the vernacular language in which communal political activity had been expressed gave way to classical Latin, a language intelligible only to an elite.<sup>3</sup> Similarly, the economic historian Armando Saporì insisted that as an economic phenomenon the Renaissance collapsed in the bank crashes of the 1340s and that the chronological period which Burckhardt had assigned to the Renaissance actually coincided with a long-term economic depression and a series of demographic catastrophes.<sup>4</sup> Indeed, Robert Lopez subsequently established a causal relationship between the collapse of Saporì's Renaissance, which David Herlihy has now documented with statistics,<sup>5</sup> and the advent of Burckhardt's Renaissance, which Lopez has defined as a purely cultural phenomenon.<sup>6</sup> These theses were also supported, at least negatively, by those historians of philosophy who simply skipped, in their histories of European philosophy, over the two centuries between the nominalists and Descartes; and those few, like Ernst Cassirer, who tried to fill in the gap with such names as Nicholas of Cusa, Marsilio Ficino, and Giordano Bruno, described an intellectual movement that admittedly led not to the physical world, but beyond it.

Indeed, the views of Thorndike and Sarton were at least consonant with what had been and what were then the leading interpretations of the Renaissance as a whole. Burckhardt himself had admitted "the defects in his

<sup>3</sup> Particularly in Ferdinand Schevill, "The Society of the Italian Renaissance," in Sarton, *The Civilization of the Renaissance*, 43-72; further exemplified in his *History of Florence from the Founding of the City through the Renaissance* (New York, 1961).

<sup>4</sup> See Delio Cantimori, "Il problema rinascimentale proposto da Armando Saporì," in his *Studi di storia* (Turin, 1959), 366-78.

<sup>5</sup> David Herlihy, *Medieval and Renaissance Pistoia* (New Haven, 1967).

<sup>6</sup> Robert Lopez, "Hard Times and Investment in Culture," reprinted in *The Renaissance: A Symposium*, February 8-10, 1952 (New York, 1953), 19-34; R. S. Lopez and H. A. Miskimin, "The Economic Depression of the Renaissance," *Economic History Review*, 14 (1962): 408-26. For Lopez's view of the Renaissance as a whole, see *The Three Ages of the Italian Renaissance* (Charlottesville, 1970).



knowledge on this point,"<sup>7</sup> and he implied by his selection of examples that the most obvious manifestation of Renaissance science consisted in the collection of strange plants and animals for the amusement of pleasure-seeking princes. Burckhardt's successors in the 1930s and 40s as the authors of the most provocative definitions of the Renaissance, Benedetto Croce and Federico Chabod, identified as its most characteristic innovation the recognition of the autonomy of certain disciplines, notably political science and esthetics.<sup>8</sup> But none of these disciplines had anything to do with what came to be known as science. And since they in turn lost their vitality, at least in Italy, long before the appearance of Galileo, they could have contributed nothing to the scientific work of the age which Croce isolated from the Renaissance under the title "Baroque."

All that was needed to make these theses wholly credible was to overcome their sole remaining defect: that of isolating Galileo from a historical context. This task was performed by J. H. Randall, who discovered a direct link between Sarton's first and second scientific ages in the University of Padua.<sup>9</sup> Padua, Randall pointed out, remained faithful to Aristotle and Averroes throughout the two centuries in which the rest of Italy was taken over by grammarians and Platonists. Moreover, it succeeded in separating philosophy from theology and in uniting it with the most experimental of the then accepted academic disciplines, medicine. Under the protection of "the leading anti-papal and anti-clerical" state of the period, the Republic of Venice, it provided Galileo with a noble lineage of nonhumanistic forefathers, from Giovanni Marliano and Ugo Benzi da Siena to Agostino Nifo and Giacomo Zabarella. The Paduan philosophers, Randall observed, developed a "combination of the resolute and compositive methods"; and they set forth a "clear statement of the procedures" that were to be adopted by the seventeenth-century scientists. Since these philosophers were all Aristotelians, he could not but conclude that "the father of modern science" was none other than "the Master of Those Who Know."

That Renaissance humanism had nothing to do with, and that it even thwarted or delayed, the birth of modern science, is a thesis that can easily be documented by passages in those humanist texts that have attracted the attention of recent scholars. If humanism begins with Petrarch, as is often held, then it bears the indelible mark of Petrarch's appropriation of Augustine's and Cicero's negative judgment of Hellenistic science. "Of what relevance is it to know a multitude of things?" asked Petrarch in the *Secretum*. "Suppose you shall have learned all the circuits of the heavens and the earth, the spaces of the sea, the course of the stars, the virtues of herbs and stones, the secret of nature, and then be ignorant of yourself?" After a two-minute

<sup>7</sup> Jakob Burckhardt, *The Civilization of the Renaissance in Italy, an Essay* (London, 1951), 178.

<sup>8</sup> Federico Chabod, "The Concept of the Renaissance," originally published in 1936 in the *Enciclopedia italiana* (Roma, 1929-39); now in English in Chabod, *Machiavelli and the Renaissance* (New York, 1965).

<sup>9</sup> John H. Randall, "The Development of the Scientific Method at the School of Padua," *Journal of the History of Ideas*, 1 (1940): 177-206; republished in Paul Oskar Kristeller and Philip P. Weiner, eds., *Renaissance Essays* (New York, 1968); and John H. Randall, *The School of Padua and the Emergence of Modern Science* (Padua, 1961).

glance at the Alps and the Rhone, Petrarch recalled Augustine's admonition: "And men go to admire the high mountains, the vast floods of the ocean, the huge streams of the rivers . . . and desert themselves."<sup>10</sup> Accordingly, all his followers for the next two centuries followed him down from Mt. Ventoux, which, as Sarton reminds us, is only a little over 6,000 feet high. If, on the other hand, humanism really begins with Coluccio Salutati and Leonardo Bruni, as Hans Baron maintains, then obviously it is concerned exclusively with establishing a new relationship between man and society and between the past and the present. What counts, for Machiavelli, Guicciardini, Gasparo Contarini, and Donato Giannotti, as well as for Bruni, is how man can control not the natural world but the human world.

Even less interest in science is to be found in the works cited to support the recent variants of Baron's definition—of those, namely, which identify humanism as a series of temporary incarnations of such eternal categories as "Republicanism" or "The Machiavellian Moment." Neither Paolo Paruto nor Niccolò Contarini had anything to say about anatomy, biology, and geography at a time when Venetian printers were making good money publishing books under such titles. Indeed, they were staunchly opposed to the kind of incipient scientific agriculture that was just then attracting the investment capital of so many backsliding refugees from maritime commerce in their day.<sup>11</sup> That the founder of natural science in Italy preferred to live under a regime which represented the very antithesis of "republican" freedom, that his most enthusiastic disciples were monks rather than merchant patricians, and that he himself was scrupulous about adhering to the liturgical formulas of the current incarnation of the medieval, hierarchical, antirepublican principle, the Counter-Reformation Church—all this merely proves once again the incompatibility of humanism and science, and it makes the latter the daughter not of Florence, but of Rome, not of the secular, but of the religious mentality of the age.

Meanwhile, those few humanists who had once been admitted to the ranks at least of the protoscientists, and those protoscientists who had been admitted to the ranks of the humanists, were expelled either from one or the other or from both. In the light of Eugenio Garin's recent additions to his still-standard biography, Pico della Mirandola can now be given credentials in science only to the extent that he was willing to read the works of the Paduan philosophers—those, that is, who, according to Randall, transmitted the scientific heritage of the Middle Ages.<sup>12</sup> For the function Pico assigned to natural philosophy would have been unacceptable both to the Averroists and

<sup>10</sup> From Book II of *Secretum and Familiares*, IV:1, in Petrarch, *Prose* (Milan, 1955), 68 ff. and 830 ff.

<sup>11</sup> Cf. Angelo Ventura, "Considerazioni sull'agricoltura veneta e sulla accumulazione originaria del capitale nei secoli XVI e XVII," *Studi storici*, 9 (1968): 674-722; and Stuart Wolf, "Venice and the Terraferma: Problems of the Change from Commercial to Landed Activities," *Studi veneziani*, 4 (1963): 415-41.

<sup>12</sup> Eugenio Garin, *Giovanni Pico della Mirandola, vita e dottrina* (Florence, 1937); and more recently, "Ricerche su Giovanni Pico della Mirandola," in *La cultura filosofica del Rinascimento italiano* (Florence, 1961). The correspondence with Ermolao Barbaro on this point is published by Garin in *Prosatori latini del Quattrocento* (Milan, 1952), 805 ff.



to Galileo. Its purpose, he insists, is to “allay the strife and differences of opinion which vex, distract, and wound the spirit from all sides,” and therefore to prepare its adherents to approach its “mistress,” “holiest theology,” one step up on Jacob’s ladder.<sup>13</sup> Paolo Toscanelli too had studied at Padua and had learned to apply mathematics and astronomy to the practical necessities of his mercantile firm at Pisa. But what he himself considered his major acquisition at the university was the friendship of the mystical neo-Platonist Nicholas of Cusa. And what he was best known for in Florence in his day was his participation in the moral, philosophical, and literary conversations at Camaldoli recorded by Cristoforo Landini. According to Garin, indeed, it was the humanistic “climate” of his native city that prevented him from becoming a Paduan natural philosopher; and it is not surprising that his calculations were more often put to the service of fantasy and prophesy than to what was recognized as science at Padua.<sup>14</sup>

Hence, Pico and Toscanelli have become humanists, not scientists, and they have been put firmly in the same category with Ficino and Poliziano, not in that of Ugo da Siena. Leonardo, on the other hand, turns out to have been a scientist, not a humanist. According to Carlo Pedretti, who is responsible for much of the recent increase in the available original texts, Leonardo owed his scientific interests, and hence his innovations even in the art of landscape painting, to “the Aristotelian teaching of the school of Padua,” which, Pedretti thinks, he must have heard about during one of his trips to Venice.<sup>15</sup> According to Manuzio Romano, a hospital director as well as a historian of medicine,<sup>16</sup> and Pamela Taylor, editor of the current English translation of the *Notebooks*,<sup>17</sup> Leonardo was “an absolutely a-historical figure”; and he owed these scientific interests to his own “insatiable curiosity,” which alone was capable of breaking through the “medieval darkness of his time.” According to Raymond Stiles, who has apparently mastered the new art of “psycho-history,” he owed them partially to his boyhood observations of the peculiar natural environment of his native village and partially to his own individual psychological makeup.<sup>18</sup> And according to all of these authorities, he owed nothing whatsoever to the human and cultural environment of his adopted city.

Thus historians may well be justified in accepting the distinction between humanism and science proposed in the 1540s by Sperone Speroni—who, almost alone among his contemporaries, happened to be both a humanist and

<sup>13</sup> I quote here from the translation of Giovanni Pico della Mirandola, *Oration on the Dignity of Man*, published in the famous collection of Renaissance treatises edited by Paul Kristeller, Ernst Cassirer *et al.*, *The Renaissance Philosophy of Man* (Chicago, 1948), 223–54.

<sup>14</sup> Garin’s biography of Toscanelli is most readily available in *La cultura filosofica del Rinascimento italiano*, and in English translation in his *Portraits from the Quattrocento* (New York, 1972).

<sup>15</sup> Carlo Pedretti, *Leonardo, a Study in Chronology and Style* (Berkeley, 1973). Cf. his *Leonardo da Vinci: the Royal Palace at Romorantin* (Cambridge, 1972).

<sup>16</sup> Maurizio Romano, *Leonardo da Vinci, anatomico ed ingegnere del moto umano* (Rome, 1971).

<sup>17</sup> Leonardo da Vinci, *Notebooks* (New York, 1960).

<sup>18</sup> Raymond Stiles, *The Sublimations of Leonardo da Vinci* (Washington, 1970). On all the foregoing, see my essay, “A Fifth of Leonardo,” *New York Times Book Review*, December 1, 1974, 2.

a Paduan Aristotelian. The essence of humanism is rhetoric, said Speroni. Rhetoric seeks to persuade by reference to what appears to be true (*verosimile*), while science seeks to establish what is in fact true (*vero*). One uses probable arguments. The other uses logically binding arguments (*argomento apodittico*). One promotes the moral improvement (*ammaestramento*) of all men in the political order of the terrestrial city. The other serves the solely intellectual interests of "those who contemplate the eternal nature of being" in the "celestial city." Hence the two are completely separate compartments of the human mind.<sup>19</sup>

Unfortunately, this extension of Speroni's distinction to the problem of the origins of modern science in Italy has been challenged recently by several new, and to some extent unexpected, events in the realm of historical scholarship. The first event is the elimination of the late medieval nominalists from their role as precursors of Galileo. The nearest that anyone has come (to my knowledge) in trying to reassert that role is Gibellato Valabrego with his observations on the similarity of certain of Galileo's statements about mathematics to certain others buried under what Valabrego calls the "scholastic subtleties" of Nicholas Oresme's *Quaestiones super Geometriam Euclidis*. But since Oresme's text was discovered only in 1951, Galileo could not have found it among the works of the Parisian doctors he supposedly read in his youth.<sup>20</sup> The similarities, therefore, are purely fortuitous—which is what Anneliese Meier long ago held to be true of all the other similarities that were once cited as evidence of direct borrowings.<sup>21</sup> Some authorities may still insist, like A. C. Crombie, "that Aristotle had a profound influence on Galileo's scientific thought."<sup>22</sup> That, after all, is obvious to anyone who has read the *Dialogue on the Two Systems of the World*, where Aristotelian cosmology is fully explained. But even Marshall Clagett now admits that medieval mechanics wrecked itself before Galileo proposed a new mechanics to take its place.<sup>23</sup> And not even Crombie denies that there was some substance in the acrimonious anti-Aristotelianism, if not of Galileo himself, then certainly of Galileo's followers, from Marco Aurelio Severino in Naples<sup>24</sup> to the academicians of the Cimento in Florence. Apparently, then, whoever persists in looking back to Paris or Oxford for the progenitors of Galilean science must now be dismissed as the victim of what John Murdoch calls the ailment of "precursoritis."<sup>25</sup>

The second event is the accession to Randall's position of Bruno Nardi and Cesare Vasoli. Aided by several able students of the history of logic, Nardi

<sup>19</sup> See Cesare Vasoli, *Studi sulla cultura del Rinascimento* (Manduria, 1968), 264 ff.

<sup>20</sup> E. Gibellato Valabrego, "Fu Oresme Precursore di Galileo?" in *Atti del symposium internazionale di storia, metodologia, logica e filosofia della scienza: Galileo nella storia e nella filosofia della scienza* (Vinci, 1967), 301-07.

<sup>21</sup> Anneliese Meier, *Die Vorläufer Galileis im 14. Jahrhundert* (Rome, 1949).

<sup>22</sup> A. C. Crombie, "Sources of Galileo's Natural Philosophy," in Maria Luisa Ringhini Bonelli and William Shea, eds., *Reason, Experiment, and Mysticism in the Scientific Revolution* (New York, 1975), 157-75.

<sup>23</sup> Marshall Clagett, "Some Novel Trends in the Science of the Fourteenth Century," in Charles Singleton, ed., *Art, Science, and History in the Renaissance* (Baltimore, 1969), 275-303.

<sup>24</sup> Analyzed in Cesare Vasoli, "Alchemy in the Seventeenth Century," in Bonelli and Shea, *Reason, Experiment, and Mysticism*, 49-58.

<sup>25</sup> John Murdoch, "Philosophy and the Enterprise of Science in the Later Middle Ages," in Y. Elkana, ed., *The Interaction between Science and Philosophy* (Atlantic Highlands, 1974), 51-74.

and Vasoli are now the leading authorities on the school of Padua.<sup>26</sup> Notwithstanding certain similarities, Vasoli says, Paduan Aristotelianism, and particularly the Aristotelianism of one of its most illustrious representatives, Nicoletto Vernia, was wholly incompatible with Galilean philosophy. It was so first, because of its insistence upon such un-Galilean doctrines as the eternity of the world, and second, because of its categorical exclusion from philosophy of "any practical function" and of "any compromise with the inferior reality of the human world." What interested the Paduans, insists Vasoli, and consequently what interested the Pisans, the Pavians, the Neapolitans, and all the other local representatives of what he has shown to have been a single Italian academic community, was not what nature was, but what Aristotle said it was. And thanks to the availability of the original texts both of Aristotle himself and of his Hellenistic commentators, they were very successful in pursuing that interest. But differences of opinion over just what Aristotle meant and over the relative value of his Hellenistic and Arabic commentators in elucidating his meaning soon divided the Aristotelians into opposing camps; and the resulting divisions were exacerbated by rivalries for academic chairs, by an increasing violence of language, and by varying preferences for one or the other of Aristotle's works as the basic text in logic. The century opened with the fight between the orthodox Averroist Marcantonio Zimara, whose appointment at Padua Pietro Bembo tried to block, and the Alexandrines Alessandro Achillini and Pietro Pomponazzi, who accused Averroes of having plagiarized Simplicius. The century ended with Zabarella's largely unwarranted attack upon Girolamo Balduino, from whom he lifted many of his main theses, and with the attack upon Zabarella by the "Platonizing" Aristotelian Francesco Piccolomini and by Piccolomini's contentious pupil, Bernardino Petrella.

To be sure, this quarreling stimulated a plethora of ingenious speculation, some of which in turn produced theses that bear a striking resemblance to those of Galileo—Balduino's reduction of logic to an "art" and Zabarella's identification of logic with the operation of the physical world. It even, on occasion, produced a positive evaluation of inductive, as opposed to deductive, reasoning that at least paralleled the positions of such anti-Aristotelian logicians as Rudolph Agricola and Peter Ramus. But the similarities have turned out, on closer examination, to be purely fortuitous. Far from freeing it for eventual application elsewhere, the separation of philosophy from theology and then from metaphysics simply sterilized it—or, in the words of Antonino Poppi,<sup>27</sup> "closed it in the old forms of bookish learning isolated either from experimental observation or from the mathematical formulation of objective data." Hence, it can in no way "be considered as an immediate precursor of [the work of] the Pisan scientist."

<sup>26</sup> Cesare Vasoli, "La scienza della natura in Nicoletto Vernia," in his *Studi sulla cultura del Rinascimento*, 241–56. In general, see Eugenio Garin, "L'Aristotelismo da Pomponazzi a Cremonini," in his *Storia della filosofia italiana* (Turin, 1966), vol. 2: ch. 1; Brumo Nardi, *Saggi sull'Aristotelismo padovano dal secolo XIV al XVI* (Florence, 1958); and *Studi su Pietro Pomponazzi* (Florence, 1965).

<sup>27</sup> Antonio Poppi, *La dottrina della scienza in Giacomo Zabarella* (Padua, 1972).

Its association with medicine had a regressive rather than a progressive effect, for it trapped the physicians in the “dogmatic naturalism of the Galenists” against which the first of the real moderns, Vesalius and Fabrizio of Acquapendente, rose in rebellion—as A. Corsano noted in 1962.<sup>28</sup> Hence, “it cannot possibly have contributed in any way to Galileo’s great methodological revolution.” Even Zabarella’s distinction between *demonstratio quia* and *demonstratio propter quid* remained firmly anchored on the syllogism, and it was aimed not at establishing the veracity of new knowledge, but solely in “restoring the classical and medieval structure of science.” It represents, therefore, not the beginnings of a new science, but “the farthest possible development” of a science which, having reached “the last step in its evolution, . . . had already exhausted itself.” “The thesis of a continuity between . . . the Peripateticism of the Cinquecento logicians . . . and the methodology of modern science,” concludes Giovanni Papuli,<sup>29</sup> “must be completely rejected.”

Apparently, then, the only contribution to modern science that can still be attributed to the Paduan Aristotelians is that of having laid bare all the inherent weaknesses and contradictions of the Aristotelian cosmos just at a time when it was becoming ever less compatible with an ever-increasing body of empirical data. For over a hundred years they had managed to put up with what Shlomo Pines has shown to be the cosmological implications of Ptolemaic astronomy.<sup>30</sup> For some fifty years they had managed to ignore the forceful, if rather obscurely stated, cosmological theses of Copernicus. But by 1600 the old cosmos had been so thoroughly explored, and its principles had been so soundly shaken by recent observations in biology (for example, Andrea Cesalpino), in anatomy, and above all, in mechanics and astronomy, that it collapsed, in Nardi’s words, “like a house of cards” the moment Galileo thought fit to subtract from philosophy the task of “saving the phenomena.” It is thus not surprising, as Angelo Crescini has shown,<sup>31</sup> that Galileo’s act provoked the unanimous hostility of all the Aristotelians of all persuasions. Nor is it surprising that they expressed their defenselessness by resorting to force—by appealing to political and ecclesiastical authority in order to censure Telesio, to condemn Galileo, and to break up the new order of the Scolopians, who had dared teach Galilean physics to school children. By the middle of the seventeenth century there was nothing left of one productive school of Paduan Aristotelians but the tiny group of aristocratic “Libertines” whom Lorenzo Magalotti at the time and Giorgio Spini more

<sup>28</sup> A. Corsano, “Lo strumentalismo logico di I. Zabarella,” *Giornale critico della filosofia italiana*, 41 (1962): 507–17.

<sup>29</sup> Giovanni Papuli, *Girolamo Balduino: Ricerche sulla logica della Scuola di Padova nel Rinascimento* (Manduria, 1967).

<sup>30</sup> Shlomo Pines, “Philosophy, Mathematics, and Concepts of Space in the Middle Ages,” in Elkana, *The Interaction between Science and Philosophy*, 75–90.

<sup>31</sup> Angelo Crescini, *Le origini del metodo analitico: Il Cinquecento* (Udine, 1965). For Cremonini’s position, see Maria Assunta Del Torre, “La cosmologia e l’inedito ‘De Coeli Efficientia,’” in her *Studi su Cesare Cremonini* (Padua, 1968).

recently have decried as the chief agents of “decadence” in the Age of the Baroque.<sup>32</sup>

The third event is the appearance of a new interpretation of what has hitherto been considered, at least in the writings of Alexandre Koyré, the only serious rival to Paduan or Parisian Aristotelianism in their claims upon Galileo’s ancestry: Platonism. According to this new interpretation, the final product of Platonism, or rather of the Neo-Platonism that issued from the translations and commentaries of Marsilio Ficino, was not Galilean science. It was rather the magnificent, grandiose cosmological systems excogitated by the great metaphysicians of the late sixteenth century, of which those of Telesio, Patrizi, Bruno, and Campanella are well known in the English-speaking world through the essays of Paul Oskar Kristeller.<sup>33</sup> Like the Aristotelians, the metaphysicians too occasionally came up with theses that sound Galilean—most notably those of Bruno concerning the infinity of the universe and the heliocentric structure of the solar system. But such theses were wholly un-Galilean. For they were based on metaphysics, on logic, or on magic, not on observation and mathematics; and they were held together by love, or by what Frances Yates calls “magical animation,” not by laws.<sup>34</sup> Hence, the first contribution of Renaissance Platonism to the rise of Galilean science now appears to have consisted in reinforcing the impression, by its proposal of alternatives, that the cracks in the Aristotelian cosmos had grown so wide as to make it unsalvageable. That, in any case, is Paul-Henri Michel’s explanation of the violence with which Bruno rejected all he had learned as a disciple of the Aristotelian Dominicans—and of the lay Aristotelian university professor Balduino—at Naples.<sup>35</sup> The second contribution consisted in suggesting to Galileo the cosmological implications of his particular studies in mechanics and astronomy. That, at least, is Garin’s explanation of the Ficinian language in Galileo’s letter of 1615 to Pietro Dini. This suggestion led Galileo to go back beyond the speculations of the metaphysicians to the cosmos of Copernicus, which he, unlike Bruno, was able to understand. What he found turned out to be as different from the Platonic cosmos as it was from the Aristotelian—and, better yet, with the aid of the telescope it turned out to be amenable to empirical verification, as Bruno’s, according to Hélène Védérine, most certainly was not.<sup>36</sup>

These contributions were then unwittingly furthered by the bungling Holy

<sup>32</sup> Giorgio Spini, *Ricerca dei Libertini* (Rome, 1950). All the current (to 1973) bibliography on Magalotti, and on the Florentine Galileans mentioned below, is listed in the bibliographical note to Book IV of my *Florence in the Forgotten Centuries* (Chicago, 1973).

<sup>33</sup> For example, Paul O. Kristeller, *Eight Philosophers of the Italian Renaissance* (Stanford, 1964). For Telesio, see Vasoli’s introduction to the offset edition of Bernardino Telesio, *Varii de Naturalibus Rebus Libelli* (Hildesheim, 1971).

<sup>34</sup> Frances Yates, “The Hermetic Tradition in Renaissance Science,” in Singleton, *Art, Science, and History*, 255–74, which, if I understand it correctly, goes well beyond the author’s position in *Giordano Bruno and the Hermetic Tradition* (Chicago, 1964).

<sup>35</sup> Paul-Henri Michel, *La cosmologie de Giordano Bruno* (Paris, 1962).

<sup>36</sup> Emile Namer, “L’Univers de Giordano Bruno,” in *L’Univers à la Renaissance: Microcosme et macrocosme: colloque international tenu en octobre 1968* (Bruxelles, 1970). Hélène Védérine, *La conception de la nature chez Giordano Bruno* (Paris, 1967).

Office bureaucrats who thwarted the publication of Patrizi's *Nova de Universa Philosophia* in the 1590's, about which Luigi Firpo has written at length.<sup>37</sup> For in deliberately discouraging the excogitation of still another metaphysical, qualitative, animate cosmos, they prepared the way for the replacement of all of them by a mathematical, quantitative, material cosmos, one which had the distinct advantage (as Galileo pointed out in his letter to the grand duchess) of guaranteeing theologians immunity from the theses of the physicists. The difference between the new cosmos and all its predecessors was evident to the antimetaphysical academicians of the Cimento in the 1660s, when they fell asleep while Orazio Rucellai read to them from his interminable Neo-Platonic dialogues. And it is just as evident to the modern reader of the descriptions of each of them presented in a symposium published in 1970 with the title *L'Univers à la Renaissance*.<sup>38</sup> As Thomas McTighe concluded succinctly, "Galileo was no Platonist. He was his own man."<sup>39</sup> True, two new ways of saving Platonism for science have recently been proposed. The first is to turn the metaphysicians into something called "empiricists" and then to make Platonism into an atemporal football, one which can be passed from "men like Ficino and Pico" (who therefore becomes a Platonist) directly into the hands of the Cambridge Platonists.<sup>40</sup> The second is to incorporate Platonism into something called "Rosecrucianism," which, because of its cryptic nature, can be attributed to almost everyone during the two centuries after Leonardo. But the first way skips the Galileans entirely. And the second still lies buried in certain "vast sleeping tomes"—tomes of which Frances Yates suspects the existence, but which have unfortunately escaped the notice of the diligent editors of the *edizione nazionale* of Galileo's complete works.

The most disturbing consequence of these three historiographical events is that they have once again left Galileo isolated from a historical context. In isolating Galileo, moreover, they have also disinherited the whole productive and multifarious school of science in seventeenth- and eighteenth-century Italy—a school which, alas, is still known only through a few scattered monographs. For the members of this school, whatever may have been their particular specialization, all derived either directly or indirectly from Galileo—the Accademia del Cimento, which was founded by Galileo's immediate disciples in Florence, the Accademia degli Investiganti of Naples, which was converted from Campanellan to Galilean science by Giovanni Antonio Borelli, and the "unbroken series of students" who, according to Luigi Belloni, passed Galileo's methods on from Borelli's disciple Malpighi to the generation of Morgagni.<sup>41</sup> Worse yet, the whole of the Italian Enlightenment, to

<sup>37</sup> Partially translated as Luigi Firpo, "The Flowering and Withering of Speculative Philosophy—Italian Philosophy and the Counter Reformation: The Condemnation of Francesco Patrizi," in my book, *The Late Italian Renaissance, 1525–1630* (New York, 1970), 266–84.

<sup>38</sup> *L'Univers à la Renaissance: Microcosme et macrocosme* (Brussels, 1970).

<sup>39</sup> In Ernan McMullin, ed., *Galileo, Man of Science* (New York, 1967), 381.

<sup>40</sup> George Boas, "Philosophies of Science in the Florentine Renaissance," in Singleton, *Art, Science, and History in the Renaissance*, 239–54.

<sup>41</sup> Luigi Belloni, "Marcello Malpighi and the Founding of Anatomical Microscopes," in Bonelli and Shea, *Reason, Experiment, and Mysticism*, 95–110, among his many studies of Italian science in the seventeenth century.



which so much study has been dedicated of late, has been divested of what Croce once established as its bond with the Renaissance.<sup>42</sup> Since the Enlightenment began as a rebellion against the literary and juridical aspects of current Italian culture, that bond could be provided only by the scientific aspects, as the eighteenth-century literati themselves admitted in giving to the Galileans the credit for having saved Italian literature from the assaults of the Marinists. If the Galilean heritage can now be traced no further back in time than to Galileo himself, then even the conscious effort of Italians in the age of Muratori to revive the poetry, piety, and historiography of the age of Della Casa, Sigonio, and the Council of Trent, must be attributed solely to cultural stimuli from abroad.<sup>43</sup> And that is an attribution which few historians would any longer wish to sustain.

One way out of this dilemma, certainly, is to ignore it—which seems to have happened at the international symposium on Galileo held at Florence in 1964. Another way is to revert to a genius theory of historical causality, something like the one Giorgio Vasari used to explain changes in art styles from Giotto to Michelangelo. But since neither of these ways is particularly consonant with the professional obligations or the acceptable methodological premises of modern historiography, only one way remains—namely, to explore once again the relation between Galileo and the first generation of Galileans on the one hand and, on the other hand, what had been the dominant element in Italian culture for the preceding two centuries. This is the way proposed by one of the leading authorities on Italian humanism, Eugenio Garin, in the essays included in the volume translated into English as *Science and Civic Life in the Renaissance*.<sup>44</sup> It is the way indicated by the recent students of Italian art and literature, who have at last filled the chronological gap left by Chabod's definition of the Renaissance between the generations of Machiavelli and Galileo. It is also the way suggested by the Galileo specialist Stillman Drake<sup>45</sup> and by the authority on all Renaissance science, Marie Boas Hall, in her still indispensable survey, *The Scientific Renaissance*.<sup>46</sup>

To be sure, it still cannot be said that humanism was specifically scientific in any of its successive stages. Man, and what man made, remained the principal object of humanistic investigation. Philology remained the principal method by which such investigations were carried on. And rhetoric, that is, the manner in which the results of the investigations were expressed, remained quite as important as the results themselves. Nevertheless, it may now be possible to say that humanism facilitated the birth of Galilean science—at least in providing the particular forms assumed by the scientific revolution in

<sup>42</sup> See in particular Benedetto Croce, "A Working Hypothesis: The Crisis of Italy in the Cinquecento and the Bond between the Renaissance and the Risorgimento," first published in 1939 and now in English in my book, *The Late Italian Renaissance*, 23–42.

<sup>43</sup> Relevant bibliography in my "L. A. Muratori e gli storici italiani del Cinquecento," in *L. A. Muratori storiografo: Atti del Convegno Internazionale di Studi Muratoriani, Modena, 1973* (Florence, 1975), 227–40.

<sup>44</sup> Eugenio Garin, *Science and Civic Life in the Renaissance*, tr. Peter Munz (Garden City, 1969), now also available in a paperback edition, *Scienza e vita civile nel Rinascimento italiano* (Bari, 1965).

<sup>45</sup> Stillman Drake, "Galileo's New Science of Motion," in Bonelli and Shea, *Reason, Experiment, and Mysticism*, 131–56.

<sup>46</sup> Marie B. Hall, *The Scientific Renaissance, 1450–1630* (London, 1962), especially 23–28.

Italy. It may be possible to say, in other words, that Galileo differed from Kepler and Descartes, that Torricelli differed from Boyle, and the Cimento differed from the Académie des Sciences to the extent that they were more directly the heirs of the humanists.

First of all, humanism from the very beginning was hostile not to the kind of science later represented by Galileo, but rather to the very kind he did so much to overthrow. When Petrarch laughed at those of his contemporaries who pretended to know “about birds and fishes, about how many hairs there are in the lion’s mane, how many feathers in the hawk’s tail, how elephants couple from behind and are pregnant for two years,” etc., etc.,<sup>47</sup> he was rejecting the botany and zoology of Vincent of Beauvais and Alexander Neckham, not botany and zoology as defined by the contemporaries of Francesco Redi; and he did so for the same reason that made the academicians of the Cimento laugh at Athanasius Kircher: that such knowledge was copied uncritically from unreliable authorities and could not be verified through observation. When Leonardo Bruni denounced the “Britannic sophisms” of the “modern philosophers,” he did so not only because the sophisms had nothing to do with his two major concerns, ethics and politics, but for the good Galilean reason that they had been made into ends in themselves. By distinguishing debate for the sake of debate from discussion for the sake of acquiring knowledge and identifying error, the humanists emphasized both the moral and the scientific shortcomings of standard academic practice in their day. What they questioned, in other words, was the value of debates like the one at Pavia in the mid-sixteenth century, where Girolamo Cardano boasted of having “silenced my opponent on the very first day, in the very first proposition, even in the judgment of all my rivals.” For the purpose of such debates was merely to furnish further evidence for the proposition: “Neither at Milan, nor at Pavia, nor in France, nor in Germany, have I ever found a man who could successfully controvert or dispute me within the last twenty-three years.” And consequently the actual question posed to the debaters was so insignificant that Cardano did not bother to mention it.<sup>48</sup> It is not surprising, then, that students at Pisa in the 1570s, most of whom arrived at the university after a thoroughly humanist preparation in Florence, attended the debates only to be amused—and only when they had time left over after banqueting, carousing, and putting on comedies.<sup>49</sup>

Second, while it was not antithetical to a science based on reason and experience, humanism was positively receptive to technology. To be sure, the technological innovations of the fifteenth and sixteenth centuries were not dependent upon humanism. As Eugene Rice has shown in the first chapters of

<sup>47</sup> From *De Sui Ipsius et Multorum Ignorantia*, in Petrarch, *Prose*, 712–14.

<sup>48</sup> Girolamo Cardano, *The Book of My Life*, tr. Jean Stoner (New York, 1962), 45–46. The place of Cardano in Renaissance culture is still far from clear.

<sup>49</sup> According to Filippo Sassetti in the first letters published in his *Lettere edite e inedite*, ed. Ettore Marcucci (Florence, 1855). This attitude of the students does not seem to reflect what Giovanni Cascio Pratilli finds to have been the transformation of the Tuscan universities into departments of the state: *L’università e il principe: Gli studi di Siena e di Pisa tra Rinascimento e Controriforma* (Florence, 1975).



his *Foundations of Early Modern Europe*, they were rather the products of the economic and social conditions of the commercial and industrial cities of the late Middle Ages, and they were often accomplished by artisans who had little or no knowledge of humanism. Yet in at least one city—namely, in the city in which both were the most productive, Florence—humanism and artisan technology developed side by side throughout the fifteenth century. Occasionally they met in the same person. They met first in Brunelleschi, whom Leon Battista Alberti admired equally for his technological and artistic achievements. They met most spectacularly in Leonardo. And Leonardo, thanks to Vasoli, can now be put back into history—not as a magician or as a proto-Rosicrucian, but as the most illustrious representative of two of the most important components of early Renaissance culture, the humanist and the artisan.<sup>50</sup>

The contact between these two currents became ever more frequent in the sixteenth century, when technology was formally fused with esthetics. What made Brunelleschi a great artist, said Giorgio Vasari, was not only his substitution of classical for “German” forms. It was also his success in constructing a snack bar on top of the cathedral of Florence, thus preventing the masons from descending to the street, on company time, whenever they got hungry or thirsty. What made the Last Judgment a great painting was in part Michelangelo’s invention of new ways of painting on plaster and of reconciling unity and variety. What made Benvenuto Cellini’s Perseus a great statue—according to Benvenuto himself—was partially its incorporation of a new way of casting in bronze. And, in Galileo’s own day, what made Pietro Tacca’s equestrian statue of King Philip III acceptable as a work of art was almost entirely the sculptor’s success in getting the horse to rear up on its hind legs and stay there, while its apparent center of gravity hung over a void.

Even more important, humanism provided technology with a theoretical framework, one which could give universal meaning to particular, practical innovations. It did so above all by making available the mathematical and scientific works of classical antiquity, and, more important still, by explaining and elucidating these works in such a way as to make them comprehensible, as they never had been in antiquity, to what T. S. Kuhn calls the “*honnête homme*.”<sup>51</sup> As the mid-sixteenth-century campaign to popularize humanism gradually turned artisans into humanists, they too began to collaborate with those whom Vasoli calls “mathematician-philologists of humanist formation” in producing a long series of practical technical manuals, from Giacomo Aconcio’s *De Methodo* to the *Quesiti et inventioni diverse* of 1546 and the *General trattato di numeri et misure* of 1556. These manuals sought to explain the particular cases supplied by the artisans in terms of general theorems supplied by the

<sup>50</sup> Cesare Vasoli, *Profezia e ragione: Studi sulla cultura de Cinquecento* (Naples, 1974), 479–80. Alberti’s interest in applied mathematics is described by Joan Gadol in *Leon Battista Alberti: Universal Man of the early Renaissance* (Chicago, 1969). The importance of Renaissance fine arts in the genesis of modern science is stressed in William P. D. Wightman, *Science and the Renaissance* (Edinburgh, 1962).

<sup>51</sup> T. S. Kuhn, “Tradition mathématique et tradition expérimentale dans le développement de la physique,” *Annales*, 30 (1975): 975–98.

mathematicians.<sup>52</sup> They thus elevated technology to the rank of philosophy, as it never had been in the schools. And Greek mathematics, which, according to Salomon Bochner, had been “slow, awkward, clumsy, bungling, and somehow sterile”<sup>53</sup> in antiquity, was now endowed with the potential for practical application. Mathematics permitted an invention in one field to be transferred to another—for example, to the art of fortification, which John Hale<sup>54</sup> has shown to have been one of the principal achievements of Italian Renaissance architects. It also permitted the transfer of a mathematical expression from the realm of art to the realm of physics—from a rule of perspective in painting to a new solution to a pressing problem in ballistics.

Thus the true ancestors of Galileo turn out to have been on the one hand the artisans who helped him build telescopes, and, on the other, the practical mathematicians—such as Ostilio Ricci, who was employed in Vasari’s Florentine Art Academy, and Federico Commandino, who was employed by the duke of Urbino to apply what he learned from the mathematical texts he was translating to the problems referred to him by the duke’s military captains. For it was the practical mathematicians who prepared the way for what, according to Paolo Rossi, was the most important innovation of Galileo: the union of technology and philosophy.<sup>55</sup> This was the union that he forged in his workshop at Padua and that he celebrated in his famous panegyric of the Venetian Arsenal. Unlike their academic predecessors, the Galileans remained faithful to the very humanistic thesis that had made the union possible: the thesis that contemplation is valuable only to the extent that it arises from observation and only to the extent that it ends in action. Accordingly, while they gazed into the skies, they also manufactured scientific instruments, drained marshes, regulated river beds, and laid out seaports. And they applauded while the painters of the age of Giovanni da San Giovanni and Pietro da Cortona dissolved walls and ceilings into the infinite space of the Galilean cosmos.

A third major contribution of humanism to the rise of science consists in its destruction of the method of argument from authority. This destruction took some time to accomplish, to be sure. Even the Ficinian Platonists, who, after all, were excluded from the universities until well into the sixteenth century, continued to assume in their acceptance of the authenticity of the Hermetic writings a corollary to the principle of authority—namely, that all knowledge had once been achieved sometime in the distant past. Still, Petrarch had long before rejected most explicitly one of the most universal principles of medieval

<sup>52</sup> Vasoli’s thesis (especially *Profezia e ragione*, 499) is abundantly corroborated by the specific information on this subject provided by Alexandre Koyré in his contribution to René Taton, ed., *Histoire générale des sciences* (Paris, 1957–61), vol. 2.

<sup>53</sup> Salomon Bochner, *The Role of Mathematics in the Rise of Science* (Princeton, 1966).

<sup>54</sup> Among Hale’s numerous studies of this subject, see “The Development of the Bastion, 1440–1534” in John R. Hale, ed., *Europe in the Late Middle Ages* (Evanston, 1965), 466–94.

<sup>55</sup> Paolo Rossi, *I filosofi e le macchine (1400–1700)* (Milan, 1962); now in English translation as *Philosophy, Technology, and the Arts in the Early Modern Era*, tr. Salvator Attanasio and ed. Benjamin Nelson (New York, 1970).

thought. According to that principle, certain ancient authors (or rather every sentence written by certain ancient authors) were infallible within the particular disciplines assigned to them: Cicero in rhetoric, Augustine in theology, and, of course, Aristotle in logic, politics, metaphysics, *et al.* Petrarch reduced even his favorites among the ancients to the rank of men—men who might be right most of the time, but who, like Cicero in his letters to Atticus and in his treatise on the gods, could occasionally be wrong, ridiculous, and reprehensible. Machiavelli did the same to Livy. He accepted Livy's word as long as he was narrating the deeds of the Romans. But he rejected it as soon as Livy stopped being a historian and tried to be a political philosopher as well.

This transformation of the ancients from a series of infallible statements or texts into individual, fallible, historically conditioned human beings could at times be somewhat disturbing. It placed upon modern men the complete responsibility for anything they might do or say. But it also solved the quarrel between the ancients and the moderns in Italy over a century before it broke out in France, as Hans Baron has shown.<sup>56</sup> As Machiavelli expected Lorenzo de' Medici to surpass the political achievements of Moses, Lycurgus, and Romulus, so Gaetano Contarini proved that the Venetians had already surpassed the Romans as constitution-makers, and Vasari proved that Michelangelo, as an artist, had surpassed all the ancients. Moreover, all of them believed that others in the future would surpass what the Venetian senators and Michelangelo had achieved in their own times. That meant that scientists were no longer bound by what one of their forefathers had said two millennia before. Thus Vesalius duly launched modern anatomy by breaking with Galen. And his innovation made such an impression outside strictly medical circles that it was celebrated by the humanist-painter Titian in a caricature of the Laocoön recently published by H. W. Janson.<sup>57</sup> Janson's discovery, by the way, is one more indication of the collaboration between artists and scientists during the century before Galileo. And it should be one more proof of the potential fruitfulness of collaboration between art historians and historians *tout-court* in the century we ourselves live in.

At the same time, the transformation of authorities into men multiplied the number of ancients who might have had correct answers. Who could now say that Aristotle was right when he disagreed with Democritus? And who could say that Ptolemy was right when he disagreed with Aristarchus? Moreover, this transformation gave the ancients a much more exalted role: that of proposing working hypotheses. As Polybius suggested to Machiavelli an alternative to Aristotle's six forms of government, so the fragments of ancient musical scores discovered by Girolamo Mei suggested to Galileo's father Vincenzo the possibility of substituting accompanied monody for polyphony.

<sup>56</sup> Hans Baron, "The *Querelle* of the Ancients and the Moderns as a Problem for Renaissance Scholarship," *Journal of the History of Ideas*, 20 (1959): 3-22. Cf. C. Corvini, "Il Rinascimento italiano nella interpretazione di Hans Baron," *Nuova rivista storica*, 39 (1955): 493-96; and Renato Pecchioli, "Umanesimo civile e interpretazione 'civile' dell'umanesimo," *Studi storici*, 13 (1972): 3-33.

<sup>57</sup> H. W. Janson, "Titian's Laocoön Caricature and the Vesalian-Galenist Controversy," in his *16 Studies* (New York, 1974).

Unlike authoritative statements, working hypotheses carried with them the injunction to try them out in practice: and it is in this sense that, according to William Shea, Galileo was a disciple of Archimedes.<sup>58</sup> When the working hypotheses of the man Aristotle failed to solve the problem of motion, Galileo turned to the man Archimedes—not for an authoritative answer, but for an alternative working hypothesis, which he then assumed the obligation of testing. And Galileo's disciples did likewise: while “testing and retesting,” according to the injunction of the Accademia del Cimento, the working hypotheses of the man they regarded as the greatest of all men but still a man, they considered themselves bound to modify or reject the hypotheses if they found them inconsistent with what they themselves observed with their microscopes and evacuated cylinders.

A particularly impressive example of the humanist campaign against the authority of the ancients is offered by the search for the philosophical foundations of literature, which, as Bernard Weinberg has shown, consumed so much of the energies of the Italian cultural world during the sixteenth century.<sup>59</sup> This search began with Horace, for whom poetry must be judged in accordance with the expectations of the audience. It was then completely redirected, after the appearance of Alessandro de' Pazzi's translation of the poetics in 1536, toward Aristotle, according to whom the quality of a work of literature is a function of its internal structure and hence independent of rhetorical considerations. The first authority yielded to the next only after considerable resistance. Indeed, Francesco Filippi Pedemonte and Vincenzo Maggi went so far as to line up parallel passages in order to demonstrate their compatibility. But this attempt failed as completely as did Jacopo Mazzoni's turn at the age-old game of reconciling Aristotle and Plato. After Lodovico Castelvetro published his Italian translation and commentary in 1570, Aristotle became almost as authoritative in poetics as he was in physics among the university philosophers. All the literary critic had now to do was to apply Aristotle's principles to whatever piece of literature came along. Unfortunately, just what Aristotle said, or might have said, became the subject of a hot debate—particularly with regard to such current literary forms as the madrigal, which Aristotle, alas, had not heard of. And the Aristotelians took turns tearing up Ariosto and Tasso and then tearing up each other until they succeeded in imposing on Italian literature what Weinberg called the “disastrous purification” of the *Gerusalemme liberata*.

As Aristotle's authority began to waver under the impact of the debates among the Aristotelians, it was challenged by the same Platonist who was later to offer a Platonic substitute for Aristotelian physics, Francesco Patrizi.<sup>60</sup> The result was not the triumph of one ancient authority over the other.

<sup>58</sup> William Shea, *Galileo's Intellectual Revolution* (London, 1972).

<sup>59</sup> Most notably Bernard Weinberg, *History of Literary Criticism in the Italian Renaissance* (Chicago, 1961).

<sup>60</sup> Francesco Patrizi, *Della Poetica* (Florence, 1969–71); several of his philosophic and scientific treatises have also recently been edited by Danilo Aguzzi Barbagli and published by the Istituto Nazionale di Studi sul Rinascimento in Florence. Patrizi says, “[Aristotle's] position, presented without any proof and without a clear definition of what imitation is, has always raised doubts in our mind” (2:60).

Rather, it was first the reduction of all the ancient authorities to the role of "guides," and then their rejection even in that role by those whom the critics claimed to be the chief beneficiaries of their speculative endeavors, the poets. Taking the advice Speroni had given as early as 1564, the Italian poets of the late sixteenth century reverted to the practice of imitation that had been dominant during the first age of humanism: If you want to write epic poetry, imitate Vergil, just as Vergil imitated Homer, and forget about the rules of the theorists. Eventually, indeed, the poets stopped bothering even to imitate; and the result was the "baroque" poetry of Giambattista Marino, some of which is dedicated to the celebration of Galileo's discoveries, and most of which is meant to be a practical demonstration of the superiority of Marino over Petrarch, Vergil, Bembo, and all the poets of the Renaissance and Antiquity put together.<sup>61</sup>

The fourth, and perhaps the most important, contribution of humanism to the birth of science consists in what Vasoli has identified as the redefinition of the nature and function of dialectic. As it was defined by Leonardo Bruni, by Lorenzo Valla, and most notably by Giorgio of Trebizond, dialectic was based not on the texts of Aristotle or the practice of the late medieval universities, but on the texts of Cicero and Quintilian. It was elaborated not in response to the debating traditions of the universities but in response to the particular conditions of Italian urban society.<sup>62</sup> It was therefore united with rhetoric, in accordance with the humanist principle that knowledge is sterile unless it is communicated, that demonstration is useless unless it persuades. In the quattrocento, this principle obliged the humanists to express themselves either in the language of Cicero and Petrarch or in the language of Boccaccio and Alberti's *On the Family*, depending on whether their audience was Italian or Florentine. In the Cinquecento, it obliged them to write solely in the language that the Accademia Fiorentina, on the authority of Bembo, proclaimed to be the common language of all Italians.<sup>63</sup> That is why Galileo, whose friends in the Florentine Accademia della Crusca published the first edition of their *Vocabolario* on the eve of his return to Florence, wrote his most important works in Tuscan Italian rather than in the technical Latin of the universities. And that is why, at least after he emerged from the strictly university environment of Padua, he chose to cast his writings in the form not of treatises and commentaries, but of dialogues and personal letters.<sup>64</sup> For

<sup>61</sup> On Marino, I follow Franco Croce, one of whose essays, "Baroque Poetry: New Tasks for the Criticism of Marino and of Marinism," is published in English in my *Late Italian Renaissance*, 377-400, where other titles from his considerable bibliography are cited. But current views on Marino differ considerably, as is evident in James V. Mirollo, "Mannerist and Baroque Lyric styles in Marino and the Marinisti" in the special issue entitled *Seicento Revisited* of *Forum Italicum*, 7 (1973): 318-37.

<sup>62</sup> In general, Cesare Vasoli, *La dialettica e la retorica dell'umanesimo: Invenzione e metodo nella cultura del XV e XVI secolo* (Milan, 1968), and in particular, Eugenio Garin, "La 'retorica' di Leonardo Bruni," in *Dal Rinascimento all'Illuminismo* (Pisa, 1970).

<sup>63</sup> Note Armando De Gaetano, "G. B. Gelli and the Rebellion against Latin," *Studies in the Renaissance*, 14 (1967): 131-58, and Mario Pozzi, "Il pensiero linguistico di Vincenzo Borghini," *Giornale storico della letteratura italiana*, 148 (1971): 216-94, and 149 (1972): 207-68. See now De Gaetano's *G. B. Gelli and the Florentine Academy* (Florence, 1976).

<sup>64</sup> As I pointed out in "The Florentine Background of Galileo's Work," in McMullin, ed., *Galileo, Man of Science*.

those were the forms that had been sanctioned by the humanist tradition since the time of Petrarch—not only because they had been used by the ancient rhetoricians, but also because they were rhetorically effective. Realism and the unity of space in painting permitted the viewer to associate directly with the *istoria* depicted—and hence to be moved fully or persuaded by its message. Dialogues and letters in philosophy invited the reader to participate actively in the discussion of the question proposed—and hence to accept more readily the arguments of the interlocutors.

The unity of dialectic and rhetoric also obliged the humanists to address themselves to the widest possible audience, not just to specialists. As the humanists' audience expanded from the notaries and rhetoricians of the time of Salutati to the merchant-bankers of the time of Alberti, and as it expanded finally to include the cobblers, carpenters, and druggists who attended the lectures of the Accademia Fiorentina, so, gradually, even those subjects which had previously been left to the specialists were at last made available to the general reading public—a social category, by the way, for which the humanists deserve much of the credit. Breaking down the ivory towers in which natural philosophy had been enclosed throughout the first century of humanism to some extent resulted from the new fashion among patricians of sending their sons to be educated, or “finished,” at the universities—which, after all, they supervised and paid for. It also resulted from the establishment, first at Ferrara and then at Padua, Bologna, Pisa, and Pavia, of chairs in literature and languages.<sup>65</sup> When students with a humanistic preparation began to attend lectures, and when humanistic scholars began to teach at the universities, it was inevitable that they got to know the professors in the older faculties. Eventually the humanists and philosophers who now associated as colleagues came together in common institutions, of which the best known is the Accademia degli Infiammati at Padua. In these institutions each member expounded his particular subject of study to the others; and when the philosophers spoke, they were forced, for the purposes of communication, to translate the technical language in which they usually thought into the language of the humanists, which everyone understood. Then the humanists who had attended courses in philosophy began to report on what they learned to the completely lay audiences in the new humanist institutions being founded in all the major cities of Italy—the best known is the Accademia Fiorentina.

Thus humanism added still another discipline to the five or six which Paul Oskar Kristeller says it had been restricted to before the 1540s. That is, to history, poetry, ethics, *et al.*, they added natural philosophy, or at least what the university professors of their day said about the structure and substance of the physical world. Consequently, such subjects, once reserved for the classrooms, were now discussed even in such purely literary works as Giovan Battista Gelli's science-fiction fantasies. The humanist popularizers made few

<sup>65</sup> See for example Charles Schmitt, “The Faculty of Arts at Pisa at the Time of Galileo,” *Physis*, 14 (1972): 243–72.



contributions of their own, to be sure, and they were generally unprepared to evaluate critically the theses they reported, as Umberto Pirotti has shown in his study of Benedetto Varchi.<sup>66</sup> But they succeeded nonetheless in arousing an interest in subjects that the Galileans were later to transform into sciences among the public at large—a public, insisted Alessandro Piccolomini, which included women as well as men.<sup>67</sup> That was the public to which Galileo turned for support after 1610, a public composed not of university professors, but of ordinary citizens, like Michelangelo Buonarroti, Jr., Jacopo Nardi, Cosimo de' Bardi—and, of course, Cósimo II de' Medici and Cristina of Lorraine. For these citizens were by now familiar with the questions and terms that Galileo proposed to them; and it was they, the spiritual heirs not of Nicoletto Vernia and Paolo Veneto, but of Alberti and Guicciardini, who took on the task of defending, divulging, and further developing what they received from him.

To call Galileo a humanist may be something of an exaggeration. For by the time he reached his maturity, many of the subjects to which the humanists had long been dedicated, such as history, political philosophy, and theology, had either lost their relevance or been turned once again back to specialists. Moreover, Galileo's disciples became scientists in a manner which would have been inconceivable to their humanist forebears. That is, they stuck to their researches; and whenever they felt moved to delve momentarily into other fields, as Francesco Redi did in poetry and lexicography, they took care to separate their pastimes from their professions. Yet without the background of humanism, Galileo's accomplishment would be incomprehensible. Humanism made him independent of established authority. It provided him with his audience. And it gave him his modes of expression. Indeed, insofar as he used his own poetic efforts to undermine his opponents, insofar as he adopted as his guide through the border regions between science and theology none other than Petrarch's favorite author, Augustine, and insofar as he discovered in the new science a way of escaping from the century-old conflict between philosophy and his own sincere Catholic faith, he can truly be called, if not the last of the humanists, at least a faithful heir of the humanist tradition.

<sup>66</sup> Umberto Pirotti, *Benedetto Varchi e la cultura del suo tempo* (Florence, 1971).

<sup>67</sup> Besides Vasoli, Florindo Cerreta, *Alessandro Piccolomini, letterato e filosofo senese del Cinquecento* (Siena, 1960); on which see Riccardo Scrivano, "Alessandro Piccolomini" in *La rassegna della letteratura italiana*, 68 (1964): 63–84.

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David Lloyd George:  
Land, the Budget, and Social Reform

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BENTLEY BRINKERHOFF GILBERT

THE ORIGINS OF THE BRITISH WELFARE STATE are usually attributed to the threat to the Liberals of the new Labour party in the House of Commons, to the evolving radicalism (or at least class consciousness) of the workers manifested by their demands for the overhaul of existing institutions for the care of the poor, and to the presence in the cabinet of David Lloyd George and Winston Churchill.

The force of political competition, it is argued, came from the left. The Liberals responded with social reform. The main achievement was the massive system of compulsory health and unemployment insurance.

Generally, accounts of the growth of welfare institutions stress the resolutions of Trades Union Congresses and the Labour party, the Royal Commission Report on the Poor Laws, labor delegations, and bills put down by the party in the House of Commons. That some social measures eventually were passed, it is insisted, proves the point. The accomplishment is taken as the result of the previous demand. Radical labor agitation had influenced Liberal policy. In fact, the two stand in the same relation that good science fiction writing does to NASA.<sup>1</sup>

These traditional explanations are not, to be sure, wholly wrong. Labor, of course, did have an impact. The Webbs *were* influential, although not with Lloyd George. But as is usual with simple historical explanations, they are less than half-right.

The bundle of legislation and legislative proposals usually taken as the Lloyd George welfare program did, in truth, arise from political challenge, but it was a challenge from the right, not the left. The alternative to Liberal Radicalism was not Labour Socialism but demagogic Toryism, and the Liberal enemy was not the ghost of Karl Marx, not even of Henry George, but of Joseph Chamberlain.<sup>2</sup> And the second, and major, point is simply that Lloyd George was not a social reformer in any sense that C. F. G. Masterman,

<sup>1</sup> For recent evidence that this tendency is continuing, see for instance Jose Harris, *Unemployment and Politics* (Oxford, 1972), 339 ff., concerning the Development Commission; or Arthur Marwick "The Labour Party and the Welfare State in Britain, 1900-1948," *American Historical Review*, 63 (1967): 380-403.

<sup>2</sup> This point concerning the 1909 budget, has been excellently made by Bruce M. Murray, "The Politics of the People's Budget," *Historical Journal*, 16 (1973): 555-70.



J. A. Hobson, W. H. Beveridge, or the Webbs would have recognized. Throughout his life his aim was the abolition of privilege far more than of poverty. Although he could grow publicly emotional about helpless widows and starving children, his political interest concerned the class above him, "the dukes," rather than those below.

His background was semirural. His cause, the one program he remained with throughout his life, was the destruction of the monopoly of land. His knowledge of urban problems, of trade unions, of "social reform," in the sense that the settlement-house intellectuals and the Fabians understood it, was extremely limited. Indeed, the people with whom he was comfortable, who he felt did most for the country, whom he trusted to get things done, and who he felt shared his feelings about the wasteful, dilettantish, country-house-oriented governing class, were the businessmen. He got to know them at the Board of Trade and he liked them. This accounts for his friendship with Bonar Law, which began in his early days at the Board of Trade and which would become so important during the war.

As Lloyd George left the Board of Trade in 1908 the state of the Liberal government may be fairly described as one of rising desperation, and conversely of reviving confidence and momentum among the Unionists. By-election losses showed that the landslide victory of 1906 had been much more a swing against the Tories than a sudden popular enthusiasm for any Liberal program. Moreover, it was clear that even though Chamberlain's stroke in July 1906 had removed him from public affairs, tariff reform, as he had expected it would, was capturing and inspiring his party. Between mid-January and mid-April the Liberals lost four seats, beginning on January 17 when the ancient Liberal constituency of Mid-Devon was captured by a Liberal Unionist tariff reformer and concluding three months later on April 24 with the rejection of Winston Churchill in the free-trade fortress of Northwest Manchester.<sup>3</sup>

Clearly the Liberals' needed new programs. The trouble with the government that had come into power in December 1905 was that although it contained many able men, Henry Campbell-Bannerman had no real claim on the cabinet's loyalty in the traditional sense. He could not (probably it never occurred to him) use the cabinet as an instrument of his own will as Gladstone did or as Lloyd George would do later. There were certain election pledges; individual ministers carried out projects. And when these were fulfilled or destroyed, no one knew what to do next. The issues of 1906—education, trade disputes, licensing, Chinese labor—were either on the statute books or had receded, to become again the property of the enthusiasts from whom they had emerged. When Campbell-Bannerman resigned, H. H. Asquith succeeded to a court of bankrupt geniuses and to the leadership of a discouraged party. The

<sup>3</sup> This last defeat was important not only because of the humiliation of a well-known free-trade cabinet minister, but because the Unionist winner, William Joynson-Hicks, was a Nonconformist businessman whose victory suggested that Northern Dissenters were deserting the party which they had helped so much in 1906.

Lords could feel fairly safe in rejecting Liberal measures. The Licensing Bill, land taxes, and valuation held no interest for a nation of beer-drinking, city-dwelling renters. Irish home rule and Welsh disestablishment belonged to irrelevant Celtic minorities. The live, popular issues were battleships and one item of social reform, old age pensions. Both required new taxes and were thus particularly dangerous to a government that had already lost the political initiative to a party claiming to possess a painless way of raising new money.<sup>4</sup>

It is sometimes forgotten that preferential tariffs were more than just the economic revival of old-fashioned imperialism. Chamberlain intended also to capture for his party the leadership in domestic reform as well. Import duties seemed to provide for the Unionists all the elements of political attraction that the party had lost. They could finance battleships; they could pay for welfare legislation; they would of course consolidate the Empire. But they would also revitalize decaying British industry and make Britain once again the workshop of the world. "Tariff reform," the slogan went, "means work for all."

Traditional Liberalism, reinforced by the Bank of England and the Treasury, had always held that taxes within very narrow limits should, in peacetime, be more or less fixed. Revenue would grow as trade grew. The government might borrow for emergencies, to be sure, but in peacetime the first charge on revenue was debt service, and excessive borrowing would raise interest rates, disrupt the capital market, and destroy the City's world influence.<sup>5</sup> On May 18, 1908, Lloyd George, chancellor for barely a month, circulated among the cabinet a memorandum calling attention to the danger that threatened the government from pensions and battleships, unless new taxes were found.<sup>6</sup> Four weeks later, on June 16, he moved a second reading of the first Old Age Pension Bill. Two months later, in mid-August, as soon as old-age pensions received the royal assent and Parliament had adjourned, he left England for Germany in the company of copper millionaire Sir Charles Henry. This trip has been frequently misrepresented. He did not go to look at German medical programs. He was seeking a way to finance the inevitable extension of pensions.<sup>7</sup> He had been horrified by the cost, uncertainty, and bad drafting of Asquith's bill.<sup>8</sup> Better than anyone, he knew the working man

<sup>4</sup> Conservatives were increasingly jubilant. St. Loe Strachey wrote cheerfully to Rosebery that he heard Lloyd George was "in despair" about the need for money. A. V. Dicey remarked that while he detested pensions in principle, the cost at least would bring tariff reform. Strachey to Rosebery, July 23, 1905, Strachey Papers, 5/12/12; Dicey to Strachey, July 8, 1908, Strachey Papers, 5/5/11.

<sup>5</sup> In 1910, J. S. Bradbury, then at the Treasury Office of Accounts, sent Lloyd George a paper on precisely this topic. A debt much above £750,000,000 was unthinkable. It would put banks out of business. "Memo on the Price of Consols," J. S. Bradbury, July 14, 1910, Lloyd George Papers C/14/1/6.

<sup>6</sup> P.R.O., Cab. 37/93/62 D.L.G. "The Financial Situation—This Year and Next," May 19, 1908. At this time Lloyd George anticipated a need for £9,500,000 of new money above a calculated present revenue of £155,850,000.

<sup>7</sup> Lloyd George pointed out in the 1909 budget statement that during the debates on pensions the previous year both parties had agreed that the extension of pensions was "inevitable." Hansard, *Parliamentary Debates*, House of Commons, 5th ser., 4 (April 29, 1909), Cols. 481–82.

<sup>8</sup> Lloyd George made an oblique criticism of the "highly conjectural" estimates about the cost of old age pensions in the Cabinet Memorandum of May 19. A. T. Davies, a personal friend of Lloyd George from Criccieth, records that Lloyd George told him, soon after coming to the Exchequer, that he had been

must be made to understand that the Liberal party was his friend and that the party must erase the image that Liberal programs proceeded either from a Celtic fringe or from small, narrow-minded pressure groups. But he must also find a way to pay for the new pensions. The German answer seemed to be compulsory, contributory social insurance.

"Invalidity, disability, etc. [as he always put it] pensions" were then the opening guns of the offensive that would revive party morale and would constitute the new Liberalism. Lloyd George was, to be sure, the architect and the main proponent of national insurance. But although he spoke in his budget speech of the thousands in the country in distress because of "ill-health or premature breakdown or death of the bread-winner" and insisted that the party would do something about it, he had no program ready in 1909. Nor would he have a program two years later. Social security was a goal to which he and Churchill thought the party should address itself in the future, and, more important, it would be the issue upon which both men hoped to see the party go to the country at the end of 1910 when, presumably, the comprehensive programs of invalidity and unemployment pensions would be vetoed by the House of Lords. But it was not an immediate goal, and the relatively modest extension of the Friendly Society system through a state subsidy that Lloyd George was projecting in 1909 bore little relation to what the national health scheme eventually became.

Insurance pensions were a detour in Lloyd George's reform program, a tactical step in the revival of Liberalism.<sup>9</sup> His main and continuing interest remained land reform and development. This appeared first in the budget of 1909, was for two years recessed by the struggle over the budget in the House of Lords and by National Health Insurance, and it reappeared finally only in 1912 with the Land Campaign. The first legislative installment, projected for the budget of 1914 on the eve of the war, was unfortunately declared out of order by the speaker. Within the terms of what is normally called "social reform"—that is, leaving aside temperance and Welsh disestablishment—the land was the only reform area in which Lloyd George had a personal, as opposed to a political, commitment.

It may be worthwhile to emphasize here the rather simple nature of Lloyd George's social philosophy—if his random assumptions and prejudices about the structure of the economic and political state in which he lived could be dignified by that name. In his own rural North Wales background he recognized only two classes: the landowners (with their appurtenances the priests) and everybody else. Social privilege and political power—and wealth too for that matter—proceeded from a monopoly of land. The squire could put himself into Parliament, maintain a church with a handful of parishioners,

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unable to find any Treasury estimate of the cost of pensions. Allred T. Davies, *The Lloyd George I Knew* (London, 1948), 63. Apparently no actuarial estimate was made. See Henry N. Bunbury, ed., *Lloyd George's Ambulance Wagon* (London, 1957), 71.

<sup>9</sup> Insurance was an aberration, argued H. W. Massingham just after the Act was passed. "It comes of no school; it is pure empiricism." "The Position of Mr. Lloyd George," *Nation*, Jan. 6, 1912.

run the village school, and send his son to Oxford on the backs of the renters. He paid no tax at all on unimproved and unproductive land. On agricultural land, by the twentieth century, he paid only three-quarter rates. Income tax was a shilling in the pound. Though the security of his wealth depended on the protection of the social system, he made no contribution to it. The squire knew nothing of the precarious profits of the businessman who maintained England's manufacturing power and with whom Lloyd George sympathized strongly.

Lloyd George began his political life attacking the landlords. His first election address at Caernarvon districts in 1890 had contained provisions for leasehold enfranchisement and reform of land taxes, and his first important parliamentary battle on a non-Welsh issue was his attack on Henry Chaplin's agricultural derating bill. In breach of parliamentary courtesy he pointed out that the members of the government would, by themselves, receive tax benefits to the total of £67,000.

Ten years after Lloyd George's lonely struggle against the agricultural derating act, land reform had become a fashionable cause for economic radicals. Many young Liberals who entered the House of Commons in 1906 had pledged to reform the land system. At the core of nearly every proposal lay what was then termed site-value taxation. This meant essentially the separation of the value of any improvements in a land parcel from the value of the land itself for purposes of rating and property tax. The potential impact of such a method of rating could bring sunshine into the life of even the most discouraged socialist. First of all, the tax would fall upon the ground-rent owner rather than upon the occupier. Also it could be assessed upon the basis of the highest possible use of the land, and while improvements deteriorated, land usually increased in value, thus a tax could be levied on the increment. Finally, the tax would penalize the withholding from the market of unimproved land and so would bring down land prices. The first step, the key to site-value taxation, indeed to all land reform, was a nation-wide valuation of land separate from revenue-producing improvements. Proposals for such bills had been included in the king's speeches of 1907 and 1908. Only those for Scotland had been proceeded with, however, and both of these had been caught and destroyed by a sharp-eyed House of Lords.

Lloyd George understood few of the theoretical implications of site-value taxation when he joined the government in 1905, although he used land slogans regularly—largely those that dealt with Welsh grievances, leasehold enfranchisement, and taxation of mineral royalties. Sophistication in these matters followed from his friendship with C. F. G. Masterman, a member of the United Committee for the Taxation of Land Values, who, in common with many others, had included taxation of land values in his election address in 1906.<sup>10</sup>

<sup>10</sup> "C[harles Masterman] has been helping L.G. with the Land Clauses in the budget and has gotten very fond of him . . ." (Dated, clearly incorrectly, May 31, 1908). Lucy Masterman, *C. F. G. Masterman* (London, 1939), 1929.

Except for the land clauses, we need not discuss here in any detail the general process of formulating the 1909 budget. Everyone knew that the great weakness of free-trade philosophy was government finance. Gladstonian Liberalism did not simply uphold the principles of peace and retrenchment; it depended upon them. Economy in expenditure was more than a principle. It was a necessity. But by the autumn of 1908 the Germans had begun a dreadnought program, and taxation in Britain was about to be saddled with an old-age pension system so badly drafted that even Lloyd George was appalled. Meanwhile, unemployment, so far as anyone could tell, was increasing. The question was not guns or butter; it was both, and the tariff reformers promised in increasingly clamorous terms the painless answer.

The budget of 1909 must be regarded as two entirely separate measures: an engine for raising vastly increased revenue; and the foundation block for the Lloyd George program of land reform. The first part was straightforward enough. The tax increases on tobacco, spirits, and pub licenses, the death duties together with two pence on the income tax, and the new super-tax would produce the enormous amount of £25,000,000, a twelve-percent increase over the previous budget and the largest amount of new money hitherto ever raised in a single budget in either war or peace. If in retrospect these measures appear ordinary, at the time they were alarming. The 1909 budget was quite simply Britain's first modern budget.

The land clauses—that is, the four land taxes, the valuation proposals, and the land development program—were, financially, the least important part of the budget. But they were the most important for Lloyd George's career. The development plan was not even in the budget, although it occupied a prominent place in the budget statement on April 29. Those who assume that Lloyd George expected the budget to tempt the House of Lords to destroy itself see land taxes as the bait.<sup>11</sup> They point to the fact that the land taxes were repealed soon after the war by the Lloyd George government and that almost immediately after the Parliament Act was passed he began to boast that he had invited the upper chamber's veto.<sup>12</sup>

Despite Lloyd George's own assertion a quarter-century later to Randolph Churchill that he had indeed introduced land taxes to enrage the Lords, the evidence shows that as in many other cases, Lloyd George's recollections of his own motives for past acts, while perhaps not exactly lies, were often a good deal less than the truth.<sup>13</sup> Land taxes, in fact, were simply a camouflage for land valuation, which was central to everything else he hoped to accomplish in land reform. Without land taxes to justify it, land valuation was simply a piece of administrative law and as such was out of order in the budget. None of the four taxes was, as it turned out, a site-value tax of the sort the reformers wanted, and Lloyd George knew that as moneyraisers they were worth

<sup>11</sup> See Frank Owen, *Tempestuous Journey* (New York, 1955), 169.

<sup>12</sup> Even so well-informed a reporter as H. W. Massingham wrote early in 1912 that "those who knew Mr. Lloyd George's mind in those days knew also that he foresaw and planned a first rejection by the Lords and endorsement by the country in the following attack on the veto. . . ." *Nation*, Jan. 6, 1912.

<sup>13</sup> See Randolph Churchill, *Winston Churchill* (Boston, 1967), 2:312.

nothing.<sup>14</sup> He boasted of this to Lord Riddell in 1912, although Riddell at the time did not understand what he was saying. "I knew the land taxes would not produce much," said Lloyd George. "I only put them in the budget because I knew I could not get valuation without them."<sup>15</sup> This was not a customary Lloyd George rearrangement of the facts after the event. Six weeks before the budget was introduced, he had been most explicit on the political problems involved in land valuation.

On March 13, 1909, he addressed his Cabinet colleagues in a memorandum:<sup>16</sup>

It is now clear that it would be impossible to secure the passage of a separate valuation bill during the existence of the present Parliament, owing to the opposition of the Lords, and therefore the only possible chance which the Government have of redeeming their pledges in this respect is by incorporating proposals involving land valuation in a Finance Bill.

On the other hand it must be borne in mind that proposals for valuing land which cannot form part of a provision for raising revenue in the financial year for which the budget is introduced would probably be regarded as being outside the proper limits for a Finance Bill by the Speaker of the House of Commons. I have consulted Sir Courtney Ilbert on the subject, and he is distinctly of the opinion that, unless it is contemplated to raise substantial revenue during the year, valuation clauses would be regarded by the authorities in this House as being a fit subject for a separate bill, and not for a Finance Bill.

At this stage Lloyd George had not yet settled upon precisely what land taxes he intended to impose. Beyond those on mineral royalties and lease reversion, he still hoped to put in a small tax on land values. Still less did he discuss with his colleagues what he intended to do with the money. As in all his reform plans, land reform was an ideal, a goal to be reached—a Utopia he would recognize when he arrived there. Farming would be made more profitable; the economic situation of the laborer would be improved; and the landlord would pay for it all. The thousands of acres that had gone out of cultivation since the 1870s and been turned into deer parks and pheasant coverts would be returned to their socially useful—and profitable—condition. But how this all was to be done, how these dreams were to be turned into legislative reality, Lloyd George never made clear because he did not know. His line of political action was always attack, and his focus was on the class above, not the one below. He was a *Frondeur*, not a philanthropist. He also had an intensely practical and empirical view of politics. He had ideals, but made no plans. Though he enjoyed working out political problems, the details of hypothetical legislative programs and indeed even the reading of other people's proposals—the Royal Commission on the Poor Laws, for instance—

<sup>14</sup> He fought unsuccessfully until the middle of March to include a site-value tax on already improved land. Opposition came from the lawyers in the cabinet on the ground of interference with existing contracts. See P.R.O. Cab. 37/98/45 "Land Value Tax," March 16, 1909. For Lloyd George's comments on the contract objection see P.R.O. Cab. 37/97/16 "Taxation of Land Values," Jan. 29, 1909.

<sup>15</sup> Lord Riddell, *More Pages From My Diary, 1908-1914* (London, 1934), 65, May 27, 1912.

<sup>16</sup> P.R.O. Cab. 37/98/44 "The Taxation of Land Values," March 13, 1909.



bored him. He could not be made to read a Blue Book. As a result, as both the National Insurance Act and the land program showed, the final state of any bundle of plans was likely to bear little relation to the original scheme. As long as it moved in the right direction, he was content.

Thus the land-reform program that would have grown from site-value taxation can only be guessed at. There are the proposals in the budget of 1914 and the land campaign itself, but the only evidence in the budget besides the statement in Chapter 8 that one-half the proceeds of land-value duties were to be appropriated to the local authorities "in a manner to be determined by Parliament," is in the now-forgotten Development Commission.

Land development received extended treatment in the budget statement, or rather, in typical Lloyd George fashion, the *desirability* of land development was elaborated at length without any specification, beyond afforestation and help for agriculture, of what he hoped to do.<sup>17</sup> Where the Development Commission would get its money in the early years before the land taxes began to pay was most unclear. Lloyd George suggested in his budget statement that he expected to funnel departmental surpluses, that is, the unspent portion of any year's supply vote, into land development rather than see them disappear into the anonymity of the sinking fund. He probably did not realize at the time that this would reverse a century and a quarter of fiscal tradition. But when the Development Commission was finally established in May 1910—with Lord Richard Cavendish, Vaughn Nash, and Sidney Webb among its members—it had been provided with £2,900,000 for the coming five-year period. But also by this time the Development Commission had retreated from the active, initiative-providing role that Lloyd George seemed to have envisioned in 1909, and it had become simply a review board authorized to make loans or grants of government money to support schemes for rural or agricultural development suggested by government departments, local authorities, and educational or charitable associations. In any case, during the years before the war the Development Commission displayed none of the energy or imagination that Lloyd George had advertised for it. Except for a grant of £325,000 to be spread over five years for the establishment of agricultural institutes, it did little that individual departments were not already doing. One would imagine that Lloyd George saw a future for it in the Ministry of Land that he hoped to establish at the end of the land campaign. It would supervise the broad new duties that he had intended to impose upon local authorities in the budget of 1914.<sup>18</sup>

Social reform then began for Lloyd George with land reform. He later expanded it into what he called National Development, but he never departed much from the original ideal: the nation was a vast estate that had fallen into

<sup>17</sup> Hansard, *Parliamentary Debates*, House of Commons, 5th ser., 4, (April 29, 1909), Cols. 490–95.

<sup>18</sup> The best account of the Development Commission, as Lloyd George intended it, can be found in the Liberal Party publication, *The Government's Record, 1906–13* (London, 1913), 263–66. Sidney Webb, as might be expected, saw the potentialities of the Development Commission and remained a member of it into the 1920s. On his reaction to appointment, see: Beatrice Webb's *Diary* (London, 1948), 430. The Commission still exists.

the hands of a selfish few who declined either to make proper use of its resources themselves or to allow others to do so. Hence, he argued, agriculture declined while the nation imported food. Mining and quarrying provided neither safety for the miners nor cheap fuel for industry, only royalties to the landowner. Capital flowed abroad or froze itself into urban land speculation, forcing slum rents so high that landlords had neither the inclination nor the need to improve. Meanwhile, British industry languished, starved for capital.

The landlord—there was the enemy. Idle land in the hands of idle men. The wealth represented there belonged rightfully to the nation, to the workers, and always also to business enterprise, to those men who had made Britain the workshop of the world and who, during the War, had flocked into the Admiralty, the War Office, and above all the Ministry of Munitions.

After the Armistice, Lloyd George took up the case again. The much-misquoted reconstruction speeches before the Coupon Election were, if carefully read, about the rural revival. The land fit for heroes was to be a new English countryside. Agricultural regeneration was central to everything else, to the solution of all British problems of industrial and physical efficiency. “An intelligent agricultural policy,” he said at Wolverhampton on November 24, 1918, when he articulated the “land fit for heroes” phrase for the first time, “is the basis of a great industrial policy, and a systematic effort must be made to bring people back to the land. That is the place to grow strong men. The touch of the soil reinvigorates and re-enforces. When there are any signs of exhaustion, bring them back to the motherland, and the old life that is in the veins of Britain flows through them, and you will find them reinvigorated and strong. Give back the people, as many as you can, to the cultivation of the soil.”<sup>19</sup> Make agriculture profitable: free frozen capital tied up in wasteful and dangerous foreign speculation, and urban industrial and housing problems would be solved.

The tragedy of Lloyd George is not his wasted interwar political career, not the tarnish of petty corruption that he added to Georgian politics, not his reputation, well-deserved as it was, for lack of principle. The tragedy is that when he did tell the truth, when he called upon his countrymen to remember that the land needed them, no one listened. And when he said he loved it also, no one believed him.

<sup>19</sup> *London Times*, November 25, 1918.



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## Reviews of Books

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### GENERAL

MICHAEL HOWARD, editor. *The Theory and Practice of War*. Reprint. Bloomington: Indiana University Press. 1975. Pp. vi, 374. Cloth \$12.50, paper \$3.95.

For a generation after World War I, B. H. Liddell-Hart was the most distinguished military critic writing in English. In 1965 one of the most distinguished of his successors, Michael Howard, edited in his honor this testimonial volume. Most *Festschriften* tend to be unreviewable grabbags of unrelated pieces of uneven quality, but the essays in this collection possess a common theme—the difficulty of thinking clearly about the nature and conduct of warfare in a period of rapid change. Most achieve a high standard.

The work is organized in three sections. The first, and most successful, deals with the attempt of nineteenth-century military thinkers to come to terms with the effects of nationalism and industrialization on war. There are essays by Howard himself on Jomini, Paret on Clausewitz, Craig on Austrian civil-military relations, Luvaas on European military thought after 1870, and Bond on the role of the British cavalry before 1914. All are excellent, and they show, in various ways, how difficult it was for the military to anticipate the impact of profound technological and political changes on war, how easily innovative and imaginative officers were hobbled, and how catastrophically the work of the most thoughtful of them was misunderstood and misused. While the fundamental character of warfare is determined by technology, these essays provide eloquent reminders that ideas play a crucial role in the world of force.

The second section, on the interwar years, presents accounts of American and British grand strategy, German military doctrine, and two rather rueful memoirs by junior officers at the time—General Beaufre on the French Army, and General Pile on the British. Both men pay tribute to Liddell-Hart's prescience. The central military problem of the day was how to avoid a repetition of the bloody impasse that had immobilized the western front in 1914–18. Liddell-Hart's solution was to

take full advantage of the newest technology (tanks and airplanes) and whatever else human ingenuity could devise (operations research, psychological warfare) to restore mobility and circumvent the deadlock. Senior officers everywhere were inclined to resist these brash notions until events—like the success of blitzkrieg in 1939–40—showed their wisdom.

In the postwar world atomic weaponry compounded the problems raised by military technology, by assuring that civilization would be utterly destroyed if there were another general war. Under such conditions, could any rational strategy be worked out? The final section, which includes essays on American policy by Kissinger and on NATO by Buchan, tries to address this ruling dilemma of current international relations. Though these pieces stand up well against a decade of hindsight, they do not show that we have made any progress since the nineteenth century in our ability to predict the character of conflict or to manage it. Students and teachers of the history of strategic thought will find this fine collection a useful supplement and corrective to Earle's *Makers of Modern Strategy*.

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FRANCO FORNARI. *The Psychoanalysis of War*. Translated from the Italian by ALENKA PFEIFER. Garden City, N.Y.: Anchor Press/Doubleday. 1974. Pp. vii, 271.

This translation of *Psicoanalisi della guerra* (originally published in Milan in 1966) introduces English readers to a leading European psychoanalyst's insights on the nagging and persistent problem of war in all cultures. In common with many behavioralists, Franco Fornari relegates traditional understanding of the outbreak of war—such forces as economic or political competition—to the secondary status of rationalizations that shield deeply unconscious "real" motives. He elaborates arguments against traditional analyses (supporting

those of Gaston Bouthoul), draws heavily from Freudian tradition, and fundamentally believes that war derives from such subconscious factors as "the paranoid elaboration of mourning."

Historians will probably react with disbelief, if not outright amusement, to this aggravating book, hailed among many European scholars as a major contribution. The chasm in methodology between historical and psychoanalytical approaches regarding what constitutes evidence, for instance, requires leaps of faith to accept conclusions that are on practically every page. One case may suffice: "Captain Cook, for example, was killed by the natives of the Sandwich Islands because he had violated one of their taboos by trying to seize the king's person. In Captain Cook the natives were punishing their own hostile feelings against their chief" (p. 53).

After reviewing psychoanalytical theory, Fornari's larger purpose is to demonstrate that neither traditional social sciences nor older psychoanalytical theory remains valid for the nuclear era. Psychoanalytical studies, following Freud's *Civilization and Its Discontents*, broadly claimed that collectivities fought, not because of avowed hatred of an enemy, but rather because of ambivalence and fear that a love object would be destroyed. Indeed, it could be argued that this vision justifies the psychological serviceability of war permitting the healthy defense of a love object. Nuclear capability, however, in which total annihilation awaits both subject and love object, forever terminates the psychological benefits of war. Hence, the author would apply the insights of his craft to uncover ways to avoid war. He simultaneously urges the importance of recognizing that this avoidance itself must include means to lift the burdens of guilt and fear.

When we reach the "practical" recommendations that are tentatively and not optimistically sketched, it is hard to avoid the impression that the sophisticated scientist has been replaced by a well-intentioned Boy Scout. In essence, the author's remedy requires individual conscious recognition of those unconscious dynamics that had caused war—a kind of mass, yet individualized psychoanalytical process. We are all guilty of participating in war when we follow our political leadership. Thus, we must devise a political or legal institution (Fornari calls it the Omega institution) to help us collectively psychoanalyze our way out of the temptation to fight. Perhaps, the author proposes, smaller satellite nations attached to the great powers might point in this direction by creating international peace-keeping forces or by merging their sovereignty in the United Nations. Fornari seems unaware that his psychoanalytically derived policy

recommendations differ very little in content or concept from traditionalist internationalist or pacifist prescriptions dating from the pre-1914 era. He is even less acute in comprehending the connection between peace and social justice.

Historians concerned with the general problem of war in society, who are willing to confront traditional Freudian analysis and who are interested in "interdisciplinary" extensions of their craft, will find this book useful as a summary of psychoanalytical theory and admirable as an example of a concerned scholar's commitment to put his knowledge into practical service. Most historians will come away with a greater skepticism of psychoanalysis as a social science. The reader's patience will be needlessly exercised by the far too literal translation and the use of words such as "dereal," when "unreal" or "illusory" would have done (pp. 141, 148). The editor and translator could have tried harder.

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RICHARD W. BULLIET. *The Camel and the Wheel*. Cambridge: Harvard University Press. 1975. Pp. xiv, 327. \$16.00.

W. H. R. Rivers pondered the fact that on an island in Oceania, which must have been reached by water, the inhabitants no longer made canoes. He wrote an article, "The Disappearance of the Useful Arts" (*Festschrift tillägnad Edvard Westermarck* [1912], 103-30), in which he wondered how common such a phenomenon might be.

R. W. Bulliet, apparently unaware of Rivers' example, set out to explain the disappearance—for over a millennium—of the wheel and the cart from the very region of their invention, the Middle East, whence they have diffused around the globe. In so doing, many other things had to be investigated as well: the domestication of the camel; the invention of harnessing and saddles; the development and decline of the incense trade; the circumstances of the integration of the Arab camel nomads into the imperial societies of antiquity; the competition between the one-humped and two-humped camels; the anomaly of certain local adjustments to the general pattern; the failure, despite efforts, to transplant the camel transport system to arid areas of Australia and America; and, finally, the impact of motorized wheeled vehicles on camel-using societies.

The story spans five millennia—from the early days of animal domestication until today—with each step succinctly told. The author characterizes his inquiry as overlapping historical anthropology

and technological history, but it has items of interest for many area and period specialists—from the Saharan Mauretanian *limes* of the Roman Empire to the regions Owen Lattimore called “the inner Asian frontiers” of China. Strict adherence to what is thematically relevant and magistral organization of discussion preserves a sense of unity despite these vast dimensions.

This is a finely documented, profusely illustrated study. Bulliet has been painstaking in the assembly of data, neither incautious nor overcautious in constructing hypotheses, judicious in evaluation of evidence (being scrupulously fair to the pro and con), delightfully lucid in exposition, and, on the whole, persuasive in argumentation. This is a book to place beside H. Pirenne’s *Mohamed and Charlemagne* and a very few others that seek to transcend a myriad of events to find the cause of epochal change.

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D. P. O'BRIEN. *The Classical Economists*. New York: Oxford University Press. 1975. Pp. xiii, 306. \$19.25.

This is a good, scholarly book on classical economics, but the relevant question for historians may be whether it is a good book for non-economists. It is written in much more straightforward and readable prose than most writings by economists, but it does deal with economic technicalities and has the equations and graphs which have become the hallmark of modern economic theory. Moreover, it is set in a basically ahistorical framework. That is, the book is not organized chronologically but by economic specialty—value theory, monetary theory, international trade theory—as they existed during the classical era. There are advantages and disadvantages to such a presentation, but those who look historically at intellectual history may find the disadvantages particularly irksome.

Substantively, *Classical Economics* has not only good scholarship but also incisive reasoning and a balanced understanding which is lacking in all too many other books on the same subject. Its technicalities are not as technical—or as historically mistaken—as those of such noted economists as Mark Blaug, Paul Samuelson, or Martin Bronfenbrenner writing on similar themes. Its readability never descends to the cute vulgarizations of Robert L. Heilbroner or Alexander Gray. It is an excellent book for a class of graduate students, or perhaps undergraduate economics majors. Whether it is equally good for those outside the field may perhaps best be determined by simply browsing through it. My guess is that J. A. Schumpeter’s *History of Economic Analysis* is a better investment for

the non-economist; maybe even for the economist as well.

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D. B. GRIGG. *The Agricultural Systems of the World: An Evolutionary Approach*. (Cambridge Geographical Studies, number 5.) New York: Cambridge University Press. 1974. Pp. ix, 358. Cloth \$19.50, paper \$7.95.

Here is a single-volume history of world agriculture. Even attempting such a feat deserves commendation, but D. B. Grigg has in fact written a splendid book. Part 1, in four chapters, is a broad stroke canvas of evolutionary forces in farming: population growth and migration, diffusion of plant and animal stock, physical environment, and economic-technological changes. Within this framework, however, industrialization created a revolution that clearly demarcates the past century from the previous one hundred centuries, as well as those countries that have experienced economic development from those that have not. The commentary is heavily detailed, ranging from water lifting devices in ancient Mesopotamia to Bougainville’s discovery of the South Pacific “Noble” sugar cane varieties in 1768.

Part 2 has nine chapters describing how particular agricultural systems came into being. Grigg wastes no time arguing over typology, choosing instead to accept, with small modification, Derwent Whittlesey’s 1936 classification of shifting agriculture, wet rice cultivation, pastoral nomadism, Mediterranean agriculture, mixed farming, dairying, plantations, ranching, and large-scale grain production. Subsections break the chapters into discussions about characteristic features of the systems, historical periods of development, and regional variations. Maps are used profusely, as are charts and tables, many from FAO publications, to provide current data.

Both the appendix and bibliography deserve mention. In a page and a half, Grigg has appended a handy reference, “Regions of Plant Domestication after C. D. Darlington.” A reader can easily find, for example, that wine grapes, *Vitis vinifera*, were originally domesticated in South West Asia. The bibliography includes 1,229 titles, which, with annotation, could probably have been published separately.

Grigg has thus performed a valuable service in making available a vast amount of information about world agriculture and in clearing the way for further study. Geographers, historians, and agricultural economists will find this book good read-

ing, useful in preparing lectures, and an excellent textbook.

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HANS-JÜRGEN PUHLE. *Politische Agrarbewegungen in kapitalistischen Industriegesellschaften: Deutschland, USA und Frankreich im 20. Jahrhundert.* (Kritische Studien zur Geschichtswissenschaft, volume 16.) Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 496. DM 74.

Admirers of Hans-Jürgen Puhle's superb 1966 monograph on the Bund der Landwirte will be disappointed with his first venture into comparative history. He sets himself the tasks of explaining the German farmers' proclivity for National Socialism while their U.S. counterparts became leading advocates of the progressive "New Deal," and of analyzing the varied agrarian responses to the decline of agriculture in industrializing societies. If Marc Bloch's description of comparative history as a "powerful magic wand" is accurate, then Puhle has missed a splendid opportunity. For his major conclusion is that the persistence of a bureaucratic, feudal, authoritarian tradition favoring revolutions from above in nineteenth-century Prussia constitutes the cardinal problem of modern German history, and is ultimately the reason for the farmers' disposition to fascism. This approach has been emphasized previously by Weber, Veblen, Dahrendorf, Barrington Moore, and Puhle himself in a study limited to Germany. Comparison with America thus has not led to the novel insights or fresh perspectives that Bloch saw as the test of the comparative method.

Conversely, Puhle views agrarian support for Roosevelt as the natural result of a thoroughgoing capitalist agriculture built around the individualist Lockean principles of property and participation, and free of premodern feudal relics. This interpretation also has a distinguished pedigree which includes Crèvecoeur's *Letters of an American Farmer* (1782) and Tocqueville's *Democracy in America* (1835). Puhle's stress on the inordinate gains made by "powerful and organized" rather than the "weak and disorganized" farmers from state intervention is similarly not a startling revelation. Seeking to emphasize the "grass roots" democratic character of American Populism, Puhle, like Norman Pollack, slights the strong southern component of the movement.

Recently, Jürgen Kocka concluded that despite the attempts of young German scholars to free themselves from traditional approaches to the past, "for all of them, politics continues to be the main *explanandum*, although more than in former times politics is explained in socioeconomic terms." This is certainly true of Puhle with the

difference that he reverses the order and reverts to using politics to understand socioeconomic developments. His indifferent success with the "powerful magic wand" was presaged in his first book when he warned against attempts to make analogies between the two agrarian movements because of the contrasting political cultures and the essentially political character of the *Bund der Landwirte*.

If one chooses, however, to read the book as descriptive parallel accounts of the sustained efforts of German and American (and to a lesser extent French) farmers to win massive state intervention to reduce the financial risks of farming by protective tariffs, price supports, import restrictions, and the gaining of parity with other economic groups, then it is both informative and suggestive. Certainly Puhle's dating of the breakthrough into "organized capitalism" by the German agrarians in the 1890s and by the Americans in the 1930s is convincing, as is his point that agriculture was more successful than labor and, indeed, the first economic sector to insulate itself from the effects of recurrent depressions by comprehensive government assistance. The exhaustive footnotes and bibliography make it, in addition, a valuable reference work for scholars interested in agrarian ideology.

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RONALD L. MEEK. *Social Science and the Ignoble Savage.* (Cambridge Studies in the History and Theory of Politics). New York: Cambridge University Press. 1976. Pp. v, 249. \$17.50.

Ronald L. Meek, Tyler Professor of Economics at the University of Leicester, believes that the four-stages theory of the development of human societies achieved near-perfect formulation in the sixth decade of the eighteenth century. He argues that the perfection of the theory of societal progress through the phases of hunting, pasturage, agriculture, and commerce was central to the socioeconomic thought of the Enlightenment. Meek maintains that the "stadial theory" of development, in emphasizing the mode of subsistence's decisive part in forming the character and customs of societies, established the paradigm upon which classical political economy and historical materialism were constructed. Accordingly, nearly every writer—from Lucretius to Adam Smith—who touched on the theory of the four stages is subjected to an explication of his texts that is problematically conducted to measure the feebleness or thoroughness of his subscription to a materialist four-stage theory.

The indigenous inhabitants of America are im-

portant because their primitive behavior, when related by travelers, provided for Europeans empirical evidence of man's original condition and the determinative function of his modes of subsistence. Meek's passion for his own theory on the happy conjuncture that made the American "ignoble savage" available to stimulate economic theory is delightfully shared with his readers. Marx was of the opinion that the French gave English materialism spirit and eloquence. Meek now proposes that the Scottish moral philosophers, especially Adam Smith and his student John Millar, were no less poetic than their French cousins and were equally profound contributors to the theory of materialism. His ignoble savages perform here a noble role in opening up a debate of significance on the origins of social science and its philosophical and anthropological beginnings.

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THEODOR SCHIEDER, editor, with the collaboration of PETER ALTER. *Staatsgründungen und Nationalitätsprinzip*. (Studien zur Geschichte des neunzehnten Jahrhunderts. Abhandlung der Forschungsabteilung des historischen Seminars der Universität Köln, number 7.) Munich: R. Oldenbourg. 1974. Pp. 196. DM 44.

The study of nationalism has been a staple of the intellectual diet of Western historians for at least a century. Here it is served up with an important variation: what happens to well-established theory about nationalism and state-building when it is applied to recent European and non-European phenomena? The authors and discussants of these essays study five European examples (Belgium, Finland, the Soviet Union, Poland, Greece) and three non-European areas (Africa, India, Latin America). The originality of their work lies in their discoveries of deviation from characteristic theory in recent European developments and in the ways that they juxtapose older and recent insights of completed European processes to developments in the non-European world, where nationalism is still emerging and state-building is still in process.

Characteristic theory based on European models sees national groups fully achieving their identity through the establishment of nation-states. The participants in this symposium are willing to use such theory as a point of departure, but are not to be bound by it. Such theory is only partially applicable to recent European or non-European developments. The authors thus focus on deviations from the norm, upon a situation where a state was built in the face of very weak national feeling (Belgium) or where nationalism was can-

celed out by ideology as the formative factor in building a modern state (the Soviet Union). Similarly, characteristic European theory is often of little help in comprehending non-European state-making. The predominant role of the bourgeoisie and the importance of language, both so characteristic of European nation-building, may throw little useful light on situations in the third world. On the other hand, real dilemmas remain in decolonized areas, where state systems inherited from the departed colonizers thwart the aspirations of tribal-cultural masses caught up in political structures not of their own making.

The authors do not pretend to resolve the theoretical problems or to produce a new, larger synthetic frame of reference. They do have a sure sense for the more important issues, and thus they contribute in moving the complex of problems toward a more fruitful level of discussion.

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D. P. O'CONNELL. *The Influence of Law on Sea Power*. Annapolis: Naval Institute Press. 1976. Pp. xv, 204. \$14.95.

D. P. O'Connell's slim volume is a provocative inquiry into the impact of international law upon the control of naval conflict situations. He argues convincingly that "navies alone afford governments the means of exerting pressure more vigorous than diplomacy and less dangerous and unpredictable in its results" (p. 3) than air or land forces. Frequently insulated physically and psychologically, coercive sea power can be used for the resolution or promotion of conflict. Populations seem to be more indifferent to violence at sea than on land where national boundaries are easily violated during military escalation; the ambiguities and increasing complexities of the law of the sea help to intensify struggles to control it. Because the law of the sea has "become the stimulus to sea power and not its restraint" (p. 13), O'Connell believes that in the future it will be essential for the most successful naval staffs to acquire a thorough appreciation of the law while planning naval operations.

*The Influence of Law on Sea Power* is a seminal work. Naval officers and students of international law will regard it as a timely contribution in an age of proliferation of maritime claims by minor powers whose small navies, nevertheless, have sophisticated and extremely deadly weapons. Students of naval history will find the historical case studies carefully constructed and balanced in interpretation. O'Connell begins with Hugo Grotius and concludes shortly before the *Mayaguez* in-



cident. He makes only slight reference to Alfred Thayer Mahan, since the intellectual father of modern sea power disregarded international law as a major factor in his classic 1890 treatise. Nevertheless, conceptually the ghost of Mahan is in the not-too-distant background of this analysis. The role of international law in naval planning is a unique focus in naval literature, and this special study is bound to be influential among naval officers and maritime lawyers.

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HARRY A. MORTON. *The Wind Commands: Sailors and Sailing Ships in the Pacific*. Middletown, Conn.: Wesleyan University Press, and University of British Columbia Press, Vancouver. 1975. Pp. xxvii, 498. \$29.95.

*The Gypsy Moth IV*, Sir Francis Chichester, and his circumnavigation of the earth gave Harry A. Morton (senior lecturer in American colonial history, University of Otago, Dunedin, New Zealand) the stimulus for *The Wind Commands*, a book on the relationship between sailors, ships, and the Pacific Ocean. The author praises the human spirit for the successful response to the greatest of maritime challenges. Section 1, "Indigenous Pacific Watercraft," and section 2, "European Ships," demonstrate in word, photographic plates, and line figures the author's conviction that beauty of craft came from the union of form and function.

Other sections deal with the incidents of Pacific maritime life. Section 3 covers the problems of navigation and maintenance. Section 4 centers on health and food. The last section touches lightly, and not very successfully, on the life of the crews—their music, commanders, broken relationships (mutinies), and cultural differences. There are forty-eight pages of notes, eighteen pages of selected bibliography, and an index.

The chronological focus is mainly the sixteenth through the nineteenth centuries. The narrative's flavor is the saltiness of ships' journals, logs, and accounts of all kinds. Important navigators are respectfully treated, although, curiously, Captain Bligh of the *Bounty* is mentioned more often than Magellan. The scholarly underpinning is essentially published journals and information gathered from visits to maritime museums. The text is strong on sailing lore (the treatment of caravels, pages 95–97, is sensitive and illuminating), and the glossary of terms is basic for the uninitiated. The list of voyages referred to in the text hints at a much broader history of the Pacific.

In composition and bookmaking, *The Wind Commands* is a graceful clipper in a larger fleet of ill-made craft. The beautiful graphics easily justify its

existence. The appeal to the general reader should be great. Why did it not appear as a trade book from a commercial press?

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LEONARD KRIEGER. *An Essay on the Theory of Enlightened Despotism*. Chicago: University of Chicago Press. 1975. Pp. xi, 115. \$9.25.

Leonard Krieger's essay is a rich, complex, and at times perplexing attempt to make sense of "enlightened despotism" as a legitimate object of historical study. It is as much an exercise in historical epistemology as a study of an eighteenth-century phenomenon. Krieger argues for reinstatement of "enlightened despotism" as an operative historical term on a new, sophisticated level. He requires that the reader discard three other choices: the compromise term, now commonly adopted, of "enlightened absolutism"; the tendency to see "enlightened despotism" as a "paradigm," that is, a concept clearly developed and applied by eighteenth-century thinkers themselves; and the inclination to use it as a "category," that is, a term we adopt retrospectively for a pattern of activity now clear to us though, perhaps, confusing to contemporaries. Instead, Krieger argues that "enlightened despotism" best serves as a Kantian "schema," an organizing concept we adopt in part from eighteenth-century language (though the term itself was used rarely, and usually in a critical context), in part from the wisdom of having seen the process of absolute rule in the name of public welfare in subsequent centuries.

This "schema," he insists, reflects both the ambiguities of eighteenth-century experience and the fundamental shifts in political attitudes from the seventeenth century. In its very self-contradictoriness—often recognized by philosophes themselves—the concept mirrored the difficulties reformers had in mediating their ideals of individual rights, constitutional guarantees, and activist reform in the name of public happiness. For Krieger, "enlightened despotism" represents a temptation, first fully experienced by the eighteenth century, to solve this dilemma by granting absolute power a conditional legitimacy—to use that power in the service of benevolent reform.

More important, however, is Krieger's insight that the concept of "enlightened despotism" brought together a decisive double shift in political attitudes: the seventeenth century treated legitimacy in terms of the origins of power; the eighteenth century did so in terms of its proper use. The seventeenth century treated constitutionalism in terms of the actual, legitimate obstacles to absolute power; the eighteenth century saw the possi-

bility that ideal constitutional arrangements could help centralize and concentrate power for benevolent purposes.

The argument is weakest when important points are asserted without adequate proof. Krieger's rejection of the term "enlightened absolutism" caricatures rather than fully refutes the logic of its proponents. His assertion that the proponents of "enlightened despotism" were closer than liberal intellectuals to the "inarticulate reading public" (p. 90) needs much more explanation and proof. And the author's sense that subsequent centuries have "patented" in practice a theoretical invention of the eighteenth century broadly and unfairly implies, without support, that there is a direct link between enlightenment thought and totalitarian despotism.

Nonetheless, Krieger's essay is generally cogent and provocative. It rewards the careful reading that its dense prose requires.

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DOMINIQUE LECOURT. *Marxism and Epistemology: Bachelard, Canguilhem, Foucault*. Translated from the French by BEN BREWSTER. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. 223. \$15.00.

NICOS POULANTZAS. *Classes in Contemporary Capitalism*. Translated from the French by DAVID FERNBACH. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. 336. \$19.50.

Although their content differs—*Marxism and Epistemology* deals with epistemology while *Classes in Contemporary Capitalism* is an analysis of modern class structure—the underlying intentions of these two books converge. Placed in the framework of modern Marxology within Western Europe, both books are neo-Leninist. Both Dominique Lecourt and Nicos Poulantzas, while recognizing the tremendous changes that have occurred within philosophy and sociology since the Bolshevik seizure of power, nevertheless seek to uphold the Leninist tradition of the unity of Marxism and natural science, as well as the Leninist heritage of class struggle and of imperialism as the finality of capitalism.

The "grey eminence" behind Lecourt is Louis Althusser. In fact, part 2 of *Marxism and Epistemology*, called "For a Critique of Epistemology," was first published in France by Francois Maspero in a series entitled *Theorie: Cours de philosophie pour scientifiques*, under the direction of Althusser. In the 1920s, a renaissance of Hegelianized Marxism erupted in Central Europe. Reacting against the growth of positivistic Marxism, rebelling against the rigidification and dogmaticization of

Marxism, Antonio Gramsci, Karl Korsch, and Georgy Lukacs rediscovered the elements of praxis, negation, and objectification in Marx. In the 1960s Althusser attempted to refurbish the Leninist heritage of Marxism. Limiting the humanism of Marx to a brief, youthful spasm in the years before 1847, Althusser offered a structuralist interpretation of Marx that again merged Marx into the scientific objectivist tradition of *Materialism and Empirio-Criticism*. Although Lecourt does not seem to define philosophy in Althusserian fashion as the class struggle on the level of theory, he does break with the Gramsci, Korsch, and Lukacs legacy and seeks instead to reconstruct the unity between Marxian and scientific philosophy—not the outmoded scientific philosophy of Mach, Avenarius, and Engels, or the retrograde materialism of Lange, but rather the empirical epistemology of Gaston Bachelard and Georges Canguilhem. Escaping the nineteenth-century version of scientific epistemology, which counterposed the subject-object, the thought-of-the-thing in contradistinction to the thing-in-itself, Lecourt embraced Bachelard and Canguilhem because both philosophers understood science as the history of the production of knowledge. Knowledge, for Lecourt, does not involve correspondence. It is rather a historical production; it is concerned with concepts, both their production and what they in turn produce. Lecourt's major accomplishment is to demonstrate the epistemological concerns of contemporary Marxism. Taking his point of departure from the twentieth-century philosophy of science, Lecourt has indicated the interconnections between Marxism and this philosophy of science, thus creatively renewing and resurrecting scientific Marxism.

*Classes in Contemporary Capitalism* illustrates the intervention of Leninism in contemporary sociological theory. Poulantzas offers, in relating the changes that technological society has wrought upon capitalism, a Marxist-Leninist interpretation of the problem of the newly emergent managerial class, an interpretation of the state in an age of the liberal interventionist state, an analysis of the C. Wright Mills white-collar service class, and a statement on modern imperialism. Nevertheless, Poulantzas is guilty of excessive theorizing. His relationization of concepts is precise, but the reader is left hungry for fresh, or even corroborative, empirical data. His most significant presentation—long overdue in American sociology—is a redefinition of the Marxian notion of class. Rather than define class in terms of income and salary, Marx, according to Poulantzas, defined class in terms of place and function in the productive process. It is unfortunate, however, that the book ends on a grievous note, a total



distortion of Marxism. In part 3, chapter 3 ("Productive and Unproductive Labor"), Poulantzas completely misinterprets Marx. Working from Marx's criticism of Adam Smith's theory of productive labor contained in part 1 of *Theories of Surplus Value*, Poulantzas erroneously takes as truth what Marx in fact annulled. Marx criticized Smith for defining unproductive labor as labor performed for revenue, and for defining productive labor as labor performed for capital. In essence, Poulantzas himself defines unproductive labor as labor performed for revenue, exactly the view for which Marx attacked Smith. A more careful reading would have shown Poulantzas that Marx himself defined productive labor as labor that increased labor power, or shortened the time needed in necessary labor. Poulantzas is forced into this egregious error by his Leninism. He needed to show the white-collar class as a potential ally of the proletariat. To accomplish this, Poulantzas must see the white-collar worker as unproductive labor, once more in the nineteenth-century format. In order to support his neo-Leninism, he is compelled to accept Smith's definition of unproductive labor. His neo-Leninism is kept intact, but at the expense of the truth of Marxism.

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PHILLIP KNIGHTLEY. *The First Casualty—From the Crimea to Vietnam: The War Correspondent as Hero, Propagandist, and Myth Maker*. New York: Harcourt Brace Jovanovich. 1975. Pp. 465. \$12.95.

This forthright study of war reportage will cheer any flagging spirit beginning to doubt the value of the historian's craft. Moreover, it emphasizes the importance of preserving documents in government archives and liberalizing research accessibility. Such observations can be flatly asserted after reading Phillip Knightley's book, which thoroughly documents the propaganda role of newspapers in wartime. From the Crimean through the Vietnamese wars, the journalistic record emerges as a dismal, repetitious tale of distortion, censorship, manipulation, omission, error, and outright lying about nearly everything—statistics, snafus, supplies, shortages, strategies. The leading as well as the lesser newspapers of the major democracies, Great Britain and the United States, participated in massive misrepresentations with the aid of editors, censors, generals, and reporters whose own objectivity often evaporated as they developed loyalties to the belligerent side that hosted them.

"I wouldn't tell the people anything until the war is over and then I'd tell them who won," remarked a Washington censor as World War II

began. Well might that spirit be seen as an encapsulation of the attitudes of most British, American, and French officials over the last 120 years. Curtains drawn over military activities, defended by the standard caveat of national security, eventually forced the best-intentioned reporter to exchange objectivity for advocacy—or depart the front. Patently, the victims of these massive deceptions have been civilian populations whose wartime patriotism is rewarded by postwar trauma after the revelation of accurate casualty figures, the unmasking atrocity stories, and the production of, in general, a truer picture of what actually happened.

In this account, Vietnam emerges as the best-reported war, an evaluation that must be understood in context. Certainly, in contrast to the non-information printed during World War I and the misinformation distributed about the Bolsheviks earlier in this century, Knightley is probably correct about Vietnam. His case is, nevertheless, weak and, as he notes himself, such a drama as the My Lai massacre could not have been cabled from Vietnam. Its dissemination was the product of work done in the United States by Seymour Hersh, an investigative reporter.

The uncomfortable revelations in these pages cannot be wished away by labeling the book as yet another leftist, antiliberal media attack. Knightley, for instance, clearly reveals the machinations of pro-leftist correspondents in the Spanish Civil War. In addition to serving as a mine of useful information, *The First Casualty* is delightfully written and relentlessly documented. A wide variety of history students could enjoy it profitably, and scholarly sensibilities ought to make space for this excellent sample of investigative journalism. It stands as a refutation of the indictment of democratic culture contained in its pages.

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THOMAS B. BIRNBERG and STEPHEN A. RESNICK. *Colonial Development: An Econometric Study*. (Economic Growth Center, Yale University.) New Haven: Yale University Press. 1975. Pp. xiii, 347. \$20.00.

This study of the dynamics of colonial development in the period from the late nineteenth century to the eve of the Second World War is a serious effort to combine econometric and conventional historical analysis. The ten countries included are: Ceylon, Chile, Cuba, Egypt, India, Jamaica, Nigeria, the Philippines, Taiwan, and Thailand.

The standard model assumes that the demand for the colony's (or quasi-colony's) exports is de-

terminated by changes in real income, domestic prices, and trade policies in the developed world, notably in the country most closely tied to the colony (for example, the U.K., U.S., or Japan). The special feature of the model is its assumption that changes in the supply of colonial exports are determined by changes in the volume of public expenditures which, in turn, are assumed to be overwhelmingly directed to expanding the colony's capacity to export. Public expenditures are taken to be a function of direct and indirect taxation of real exports and the value of imports. The circle is closed by assuming that the demand for imports is a function of real exports, export prices, and import prices. In an assumption open to some question, export prices are taken to be endogenously determined.

The model is then used to explore similarities and differences in the experience of the various countries, notably with respect to the productivity of government outlays in promoting exports and the differential impact of external events affecting the demand for their products, in particular World War I and the Great Depression. The "low productivity group" consists of Egypt, India, Jamaica, and Thailand; the "high productivity group," Ceylon, Chile, Cuba, Nigeria, the Philippines, and Taiwan. As one would expect, the countries tied to the U.S. and Japan suffered least during World War I, those tied to the U.S. suffered most during the Great Depression.

Although the variables used in the econometric calculations are highly aggregated, the historical and institutional chapters (notably, one, three, nine, and ten) capture something of the story of particular commodities, policies, and circumstances required for the numbers to make sense.

The largest question not answered in this study is whether, at their particular stage of development, the allocation of government (and other) investment resources made sense. Were these nations (like pre-1901 Australia, pre-1896 Canada, pre-1914 Argentina) whose comparative advantage lay in export development? Or did foreign control and influence distort them from an optimum path? Could (as in the São Paulo region) industrial development have been combined with a vigorous expansion of exports? The failure to get at this question stems, of course, from the narrow range of variables examined and the high degree of their aggregation. One would never know from this study that a very large modern cotton textile industry existed in India during the period examined.

*Colonial Development* is, therefore, a narrow contribution to the study of economies dependent for the bulk of their foreign exchange on food and raw material exports; but, used with sensitivity to its

constraining assumptions, it is a useful addition to the literature.

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MARTIN J. SHERWIN. *A World Destroyed: The Atomic Bomb and the Grand Alliance*. New York: Alfred A. Knopf. 1975. Pp. xvi, 315, xi. \$10.00.

A jacket testimonial by a Princeton colleague of the author, that "this is diplomatic history at its best. . . . Altogether convincing, and beautifully written," I took to be a pardonable hyperbole by a sympathetic associate. But upon reading the book, I find the statement to be not far off. Martin J. Sherwin has undertaken a study of the diplomatic consequences of the atomic bomb, and he has shown it to be of major significance for our times. He has taken a subject that tends to be dull and has made it lively and fascinating. He has ventured into an area of immense complexity and has emerged with a model of clarity.

Among the scientists who were associated with the Manhattan Project and who were concerned about the long-range implications of their work, two emerge with new significance. One was Leo Szilard, a Hungarian-born physicist who had great foresight about the development of nuclear energy, including the development of the hydrogen bomb and the need for control of raw materials and for on-site inspection as essential to any system of international control. Although he led a movement to oppose use of the atomic bomb once it had been developed, Szilard drafted the letter to Roosevelt that Einstein signed to urge the American development of the bomb. The other scientist was Nils Bohr, who made his way out of Nazi-occupied Denmark to participate in the program and, above all, to crusade for developing a program for international control of atomic energy. His work provided much of the basis for the later Lillienthal-Acheson-Baruch plan.

Bohr urged Roosevelt and Churchill to open negotiations with Stalin during the war, before the bomb had been tested or even developed, for international control. Churchill urged the retention of an Anglo-American monopoly that might serve as an instrument of diplomacy to counter the postwar ambitions of the Soviet Union, and Roosevelt accepted this view. Later Churchill changed his mind, but Roosevelt never did. Sherwin shows that Roosevelt was skeptical about the prospects of getting along with the Soviet Union after the war, and Truman later made explicit the skepticism FDR had concealed. Whether there would have been a great difference in Soviet-American relations had Roosevelt lived out his term seems doubtful. On

the old question of why Stalin expressed so little interest or surprise when Truman told him, in an offhand way at Potsdam, about the "new weapon"—whether because he already knew about it through his agents, or because he did not comprehend the significance of what he was told—it is clear to Sherwin that the former was the case.

A number of such questions, and more basic ones about policies, come in for new points of emphasis and explanation. Sherwin has carefully gone through thousands of documents, and he has examined all the relevant papers of the participants, with the exception of a few that remain closed. He has conducted personal interviews. In short, he has done the prodigious work that makes this a nearly definitive history, and makes one shudder on approaching it. Yet one comes away with a sense that relatively little has been added to the understanding and knowledge already available through a superficial study based upon the memoirs of Stimson, General Arnold, Churchill, Byrnes, and Truman; the reports of the United States Strategic Bombing Survey (1945); the official history of the United States Atomic Energy Commission by Richard G. Hewlett and Oscar E. Anderson, Jr. (1962); and the works of James P. Baxter, 3d (1946), P. M. S. Blackett (1948), Robert J. C. Butow (1954), Paul Kecskemeti (1958), and Herbert Feis (1961). More refinement will follow the opening of other papers, and new interpretations will follow with the times; but, clearly, after a mine has been worked for only a little while, further nuggets require tremendous effort.

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JAMES P. SEWELL. *UNESCO and World Politics: Engaging in International Relations*. Princeton: Princeton University Press. 1975. Pp. 384. \$18.50.

Nations have had considerable trouble working together in formal organizations. The League of Nations and other such groups have seldom lived up to the hopes and expectations of the men who founded them. In *UNESCO and World Politics* James P. Sewell seeks to use the United Nations Educational, Scientific, and Cultural Organization as an example to determine why inter-governmental organizations do not work particularly well.

Sewell examines UNESCO from its start after World War II. Numerous individuals already during the war believed that contacts between nations, which would go beyond the necessary political ones, could help avert future wars. Meetings of a number of Allied ministers of education laid the foundation for UNESCO, which was formally or-

ganized after combat stopped. Sewell describes the new agency, located in Paris, that was separate from but related to the United Nations. He characterizes the men who served successively as director-general—Julian Huxley of Great Britain, Jaime Torres Bodet of Mexico, Luther Evans of the United States, Vittorino Veronese of Italy, and René Maheu of France—as he assesses UNESCO activities, which ranged from emphasizing education as a liberating force to saving threatened monuments and exploring arid regions in hopes of recovering desert land.

Although Sewell spends two-thirds of the book dealing in detail with UNESCO, his larger purpose is to examine the general process by which international groups manage to function. He focuses on "engaging" as the way various nations become involved in continuing relationships with others in the group, and he outlines reasons those nations continue either "engaging" or "disengaging" from the tasks at hand. Sewell argues, from the example of UNESCO, that policy "is determined by those actively engaged rather than by the play of overt conflict among participants." Nations opposed to a given course of action can easily "blunt the effect of collective decisions." He concludes that UNESCO and similar organizations have not been more successful because of "the failure of critical governments to commit themselves sufficiently in following through what they had resolved to undertake."

Sewell's account is solid and readable, if not always as vivid as it might be. He deals well with the aims and aspirations of the organization, with the leaders and nations involved in its work, and with the organization's changing focus. Historians will find his assessment useful as long as they accept his own acknowledgment that today's world is less neat and more complex than his theoretical construct would seem to imply.

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## ANCIENT

I. E. S. EDWARDS *et al.*, editors. *The Cambridge Ancient History*. Volume 2, part 2, *History of the Middle East and the Aegean Region c. 1380–1000 B.C.* Third edition. New York: Cambridge University Press. 1975. Pp. xxiii, 1128. \$37.50.

This volume presents in more than eleven hundred pages the work of twenty-four scholars whose competence in their respective fields is unanimously recognized. Most of the chapters are revised versions of separate fascicles published in the 1960s. Historians who used the monographic studies then will now greatly benefit from this overall view of

the last four centuries of the second millennium B.C.

The volume underscores the pivotal character of the Egyptian, Assyro-Babylonian, Hittite, and Mycenaean cultures. In the Near East proper the Syrian territory, by geographic imperative, was a crossroads of peoples whose fortunes were determined by their peripheral neighbors. Around 1380 Tushratta, the ruler of the Mitannian kingdom, which extended over upper Mesopotamia and north Syria, attracted the respect of Amenophis III. A formal understanding must have existed between the two monarchs, for the pharaoh maintained his dominion over the coast of Syria, Palestine, and the region of Damascus, while the Mitannian king kept the rest of Syria (A. Goetze, p. 5). Later on, the victorious intervention of the Hittite king, Shuppiluliumash, in Syria put an end to the Mitannian empire. His Syrian wars and those of Murshilish II (pp. 123-26) strengthened the Hittite claim for world leadership. These international adventures coincided in Egypt with a growing lack of interest in the Asiatic lands. By the end of the reign of Akhenaten, one of the best-known Pharaohs of the Eighteenth Dynasty, the country had failed to support the independent states of south Syria with effective military aid. C. Aldred, who has admirably analyzed the character of the El-Amarna revolution, sees "a distinct antiquarianism" in the return to a more exalted status for the Pharaoh: "the increase in the power and glory of the kingship was the inevitable political concomitant of Akhenaten's religious ideas" (p. 52). After the economic chaos that Akhenaten's policy provoked, "the Edict of Horemheb" (p. 75) represented the beginning of an era of reforms. In foreign policy Sethos I of the Nineteenth Dynasty renewed the Egyptian campaigns in Syria. These military expeditions prove that Egypt held Palestine firmly under Akhenaten and his successors (W. F. Albright, pp. 98-116; Aldred, p. 85; and R. O. Faulkner, p. 218).

During this time Ugarit exemplifies the policy of ambiguity of the small kingdoms caught between Egypt and the Hittites. At the Battle of Qadesh in 1300 Ugarit sent a contingent to aid the Hittite King Muwatallish against Ramses II, but after 1284 diplomatic relations with Egypt were fully restored (M. S. Drower, pp. 133, 140-41). The precarious condition of Ugarit vis-à-vis the political powers of the day contrasts with the richness of its literary legacy. Left aside by world politics Ugarit clung all the more to its cultural achievements. In the meantime, Ashur-uballit (1365-30) became the restorer of the power of Assyria. His military intervention in Babylonia inaugurated a period of dramatic relations between the two Mesopotamian countries. The removal of the Hur-

rian pressure in Assyria and the advance of assimilation of Kassite elements in Babylonia prompted changes in the domains of law and society (C. J. Gadd, pp. 34-38). In the course of some eighty years Assyria emerged as a Near East power because of its growing economy, its military strength, and the vigorous personalities of Adad-nirari, Shalmaneser, and Tukulti-Ninurta I (J. M. Munn-Rankin, p. 274). The military triumphs of the fourteenth and thirteenth centuries encouraged the Assyrian literary activity, to which the Babylonian traditions must have given a stimulating impetus (pp. 295-98).

The epigraphical and archeological information about the period from 1380 to 1000 is naturally uneven. Egyptian and Hittite foreign policy, Assyrian achievements in the arts, or the premonarchic traditions of Israel are better known than the history of western Anatolia, the expansion of the Mycenaean civilization, or the cultures of the western Mediterranean. Compared with the detailed historical records of Egypt or Mesopotamia, the material evidence provided by Mycenaean citadels, palaces, tombs, bronze vessels, Linear B tablets, or pottery (F. H. Stubbings, pp. 165-87) is difficult to evaluate. The facile expediency of dwelling on the material remains of the Mycenaeans when a basic knowledge of their history is wanting seems a dubious way of writing history. The eastern Mediterranean elicited the political phantasies of the inhabitants of its coasts in much the same way Syria roused imperial dreams in Egyptians, Hittites, and Mesopotamians. But for all we know the Mycenaean thassalocracy did not reach the standards of a true colonization (pp. 200, 675, 721, 733, and 752).

The invasion of the sea peoples is the most important episode of the last three centuries of the second millennium B.C. Some of the new hordes first appeared as allies of the Hittites and Egyptians in the Battle of Qadesh in 1300. R. D. Barnett deals carefully with this complex problem (pp. 359-78) using "a bewildering tangle of evidence," much of it highly fragmentary and conjectural. The disruption provoked by the sea peoples, who could have been operating in the Aegean before they made their way to Syria and Palestine (V. R. d'A. Desborough, p. 668), caused the fall of the Hittite empire and the destruction of Ugarit, dismantled the Mycenaean trade, and was partly responsible for the emergence of the Phoenicians, the creation of the Hebrew monarchy, and the Greek settlement in the eastern Aegean and Asia Minor.

This book is a work of high scholarship. The authors express opinions but do not engage in pleading; they only introduce the complexity of some basic issues. The book has 125 pages of bibli-



ography, which unfortunately is not always up to date, and an extensive index. It will be of interest to specialists as well as to historians of later periods because many of the cultural and political changes that ushered in the world of the first millennium B.C. were still operating during the Greco-Roman centuries.

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College at Purchase

CARMEN BLACKER and MICHAEL LOEWE, editors. *Ancient Cosmologies*. London: George Allen and Unwin; distributed by Rowman and Littlefield, Totowa, N.J. 1975. Pp. 270. \$17.50.

This collection of essays originated from a series of lectures that specialists delivered to a general audience in 1972 at Cambridge University. In this collection cosmology refers to any more or less systematic representation of the universe, whether based on religious experience and speculation (such as Christian eschatology, the Buddhist *arūpadhātu*, or "sphere of non-forms"), empirical evidence (such as the Ptolemaic system), or more recognizably individual, creative combinations of these (such as the world of Dante's *Commedia*). The contributors and their topics are J. M. Plumley on the cosmology of ancient Egypt; W. G. Lambert on Sumer and Babylon; Rabbi Louis Jacobs on Jewish use of foreign cosmologies; Joseph Needham on ancient and medieval China; R. F. Gombrich on ancient Indian cosmologies—Vedic, Brahmanic, Jain, and Buddhist; Edith Jachimowitz on the unique Islamic synthesis of its own religious revelation and Greek natural philosophy; H. R. Ellis Davidson on pagan Scandinavian cosmology; G. E. R. Lloyd on the Greeks; and Philip Grierson on the elaborate, not always coherent Hellenic-Judaic-Christian cosmic view developed in medieval Europe and prevalent until after the Renaissance.

Considering that the severe limitations of space and the complexity of the data tax to the utmost any scholar's synthetic powers, and run the risk of superficiality, these synopses are with few exceptions unexpectedly rich. Least successful, perhaps, is the Indian material, which, in its quantity and tangled complexity, simply resists Gombrich's best efforts to compress it into thirty pages; it will require several readings. (I cannot remotely imagine it as a lecture.) More effective is Lloyd's strategy: to concentrate less on narrative exposition of particulars than on generalizing the enduring characteristics of Greek cosmological thought under the proliferation and variety of competing systems—its anthropocentrism, its dialectical nature, its rationalism, its nonpractical bent. What one

misses in this collection is a synthesizing essay, following Lloyd's model, that would not, like Carmen Blacker's introduction, merely summarize the contributions as discrete entities, but would attempt to abstract a general, cross-cultural characterization of cosmological literature and its basic structure, function, and relation to other manifestations of culture.

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JOHN G. EVANS. *The Environment of Early Man in the British Isles*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xiv, 216. \$12.95.

J. DONALD HUGHES. *Ecology in Ancient Civilizations*. Albuquerque: University of New Mexico Press. 1975. Pp. x, 181. \$9.50.

It is a commonplace that contemporary concerns are often translated into historical research. An unavoidable process, its usefulness can be immense, as the recent impact of the movements for blacks' or women's liberation on various aspects of our historical understanding demonstrates. The dangers inherent in approaching the past through the present are equally obvious. They may be summarized as the inability to grant the past its natural autonomy—the simple projection of present interests and problems back into the past—as if the very form and content of the categories of contemporary society were universal and unchangeable.

It was inevitable that increasing concern with the quality of the natural environment in the mid-twentieth century should spur historical investigation of "human ecology." J. Donald Hughes' *Ecology in Ancient Civilizations* represents an early, synthetic product of these investigations as they concern Mediterranean antiquity. Unfortunately, this volume illustrates the dangers of, rather than the usefulness of, "presentism." After brief (and extremely superficial) explorations of the "Mediterranean ecosystem" and of the relationship to nature of prehistoric and early Near Eastern (especially Hebrew) human beings, Hughes focuses on his main subject matter, the ancient Greeks and Romans, their attitudes toward nature, and their impact on the natural environment. He ends his volume with a brief chapter of uncertain purpose on early Christian views of the environment and a concluding section, "The Ancient Roots of our Ecological Crisis," the very title of which reveals the volume's purpose and limitations.

Following popular views of contemporary ecological catastrophe, Hughes has reduced ecology

to human tampering with and destruction of the environment. That there is a far wider sense is not unknown to him, for at the very beginning he defines ecology as "the study of the interrelation of living things to one another and their surroundings" (p. 2), but whenever he leaves his narrow focus on the "depleted environment" for this wider meaning of ecology, his volume descends into trivia, irrelevance, and error. In any case, his main interest lies in such subjects as deforestation, exhaustion of mineral resources, and the detrimental effects on the environment with the spread of agriculture and technological invention.

But if the subject is to have any meaning for the study of antiquity, it must be considerably broader in scope. Human beings and the natural environment cannot be separated mechanically as if they were opposing forces, simply acting upon one another in destructive ways. They are far more closely interrelated. Human beings operating within nature are constantly creating the environment that at the same time they are changing, depleting, or even destroying. This is no less true of Greece and Italy than of the more spectacular cases of the Nile Valley and Mesopotamia. Thus the process has more than one purely destructive side that needs to be investigated.

The investigation can hardly proceed on this level alone, however, for there exists no general human manner of relating to the environment. Rather, each society relates in its own manner and according to the rules of its own internal functioning. Thus, in classical Greece a major determinant of the relationship was the nature of land tenure and the normally close connection of citizenship and landholding. Comprehension of the socioeconomic institutions of Greek society would therefore have to precede investigation of the Greek relation to the natural environment, and this Hughes has failed to do except in the most superficial manner. In sum, his volume (whether intended for scholars, or, as is perhaps more likely, for students) is not very useful.

Although its general subject matter may be of less interest to many historians, John Evans' *Environment of Early Man in the British Isles* is in a different category. The product of an older archaeological tradition and far less pretentious, Evans' book deals not with human history, but with the nature of the environment in which that history was made in the British Isles. He focuses on the prehistoric period, but he offers brief accounts of the more recent periods up to the Industrial Revolution. He deals with the changing geology, soil conditions, flora, fauna, and climate of the region in various periods, and he notes the effects of these on the development of human cultures. As Evans proceeds into the Neolithic period and beyond, he

demonstrates the increasing role of human culture in creating and shaping the environment in positive as well as negative ways, until in relatively modern periods the human factor becomes almost as important as the environment itself. Evans is also aware of the importance of social and economic institutions such as landholding patterns and the like, although his main interest is not in them per se. His volume is surely far more of a model of good environmental history than that of Hughes.

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ARNALDO MOMIGLIANO. *Alien Wisdom: The Limits of Hellenization*. Cambridge: Cambridge University Press. 1976. Pp. 174. \$11.95.

In these lectures given at Cambridge and Bryn Mawr, Arnaldo Momigliano muses upon Greek awareness of Roman, Celtic, Jewish, and Iranian cultures during the Hellenistic age. Carthage and Egypt are omitted, the former for lack of evidence, the latter because there was no significant change in Greek evaluation of Egypt in the Hellenistic period. One might question the exclusion of Egypt, but Momigliano is working primarily on the plane of intellectual contacts.

From the author one expects a wide and deep knowledge of many diverse sources and of the modern bibliography (arranged for each chapter in chronological order). One finds primary attention given to the literary evidence, though Momigliano occasionally cites archeological and other physical material. As always, the author is witty and penetrating. On the Hellenistic age as a whole, in comparison to earlier centuries, he stresses that it "is tame and conservative. Until St. Paul arrives on the scene, the general atmosphere is one of respectability." Rather more than in earlier works, Momigliano passes terse but emphatic judgment on the peccadilloes of recent scholarship. It is comforting to me to find a historian who feels we can still read Mommsen with profit.

There is not a great deal to be said for the Celts. On the Romans, Momigliano is excellent, and he emphasizes the novel point that it was Roman interest that impelled many Greek scholars to explore non-Greek cultures. The chapters on the Jews are lengthy, and here the author to some degree looks at his subject not from the Greek, but from the Jewish side. Early in the discussion of Iran comes the disturbing assertion that Greeks were involved "at practically every level in the process of expansion of the Persian state," but the pages that follow are more cautious. In forthcoming issues of *Iranica Antiqua* I shall present a some-

what different view of the meeting of Achaemenid and Hellenic cultures.

For a casual reader the pages of this book are fearfully forbidding at first sight, inasmuch as references in parentheses stud the text. But anyone interested in Momigliano's subject should let his eyes slide on past the frequent citations. His well-supported arguments will repay attention even by scholars who do not specialize in ancient developments.

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BORIMIR JORDAN. *The Athenian Navy in the Classical Period: A Study of Athenian Naval Administration and Military Organization in the Fifth and Fourth Centuries B.C.* (University of California Publications: Classical Studies, volume 13.) Berkeley and Los Angeles: University of California Press. 1976. Pp. xiii, 293. \$12.50.

Athenian sea power has recently been treated by Amit in *Athens and the Sea*, a brief, descriptive study of warships, crews, and the harbor of Piraeus. Borimir Jordan analyzes at greater length the shore administration, naval officers, and crews. A student of modern navies will be amazed by his picture: foreigners contracted to serve as ship captains; returning galleys could be abandoned on the shore, and their movable equipment regained only by tedious lawsuits; one captain broke off from an expedition to take care of his farm; crews were difficult to obtain and deserted wholesale if unpaid. Yet on this foundation Athens exercised the first major command of the sea in Western history.

Jordan's dogged determination to reach fresh views at times produces interesting results, as in his treatment of generals and the subsidiary *archontes*. Elsewhere his analyses, though detailed, demand caution. For example, his initial picture of early Athenian naval activity is terse, but twice, much later, he advances an ill-founded view that clans provided the vessels. (On the role of *gene*, one must now take into account F. Bourriot's dissertation at the Sorbonne, 1975.) Jordan portrays sixth-century naval strength as considerable, yet he does not mention Herodotus' remark that Athens had to beg ships from Corinth to fight Aegina. On marines, he tends to forget that rowers also had to fight, and he relies too heavily on the battles of Salamis and Syracuse, fought in narrow waters. On archers, however, he is judicious. The term *hyperesia*, always taken to mean the skilled component of a crew, here becomes a set of slave rowers; why then should the *hyperesia* have been named specifically in a mobilization? Until the days of gunpowder, in any case, rowers at Athens

or Venice were normally free, and slaves were used only in emergencies. On this point the student should consult Sargent, Casson, or Amit to counter Jordan's interpretations of the purported evidence.

Documentation is extensive, the bibliography is full, and there are six indexes. Someone at the press should have realized that at least twice the same factual material, complete with footnotes, is repeated (pp. 63 and 70, 205 and 223).

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D. M. PIPPIDI. *Scythica Minora: Recherches sur les colonies grecques du littoral roumain de la Mer Noire*. Bucharest: Editura Academiei. 1975. Pp. 314.

The articles and essays collected here, without exception, have been published before in one form or another. Besides D. M. Pippidi's authorship, there are two principal themes unifying the collection: all the essays deal with the ancient Greek *poleis* of Histria, Tomi, or Callatis, located on the western shore of the Black Sea, south of the Danube, on the coast of present-day Romania, and they are all based on epigraphical materials. The book will interest a very small and professional audience of specialists in the history of the Hellenistic east, particularly those concerned with archaeology, epigraphy, Greco-Roman religions, and Roman expansion. The chief value of this collection seems to be the appearance in a language and form easily accessible to the general scholarly community of work originally published in languages not readily comprehended, for example Romanian, or in obscure journals not easily available in Western libraries, such as *Dacia* and *Studii clasice*.

These 25 articles cover the Hellenistic and Roman periods and focus almost exclusively on the three Greek colonies named. They discuss matters upon which the rather recently discovered epigraphical evidence sheds light, especially socio-political questions, the history of religious cults, and some economic topics. The articles depend heavily on Pippidi's previously published work on the subject, particularly his *Epigraphische Beiträge* and *I Greci nel Basso Danubio*, to which he makes constant reference. Although he says that these studies represent his most recent work, carefully revised, several chapters are rather dated (for example on p. 81, "Depuis que ces lignes ont été écrites, les sculptures ont été publiés" [in 1961!]).

The chapters themselves are organized more or less along chronological lines, but there is much repetition (since the same inscription is often discussed in different contexts where it provides information on different subjects), leading to a sense of



redundancy. Just as the entire collection lacks tight organization, so also individual chapters often contain insufficiently developed arguments, topics intruded in an annoying or confusing fashion and, on occasion, the danger of circular reasoning based on analogies with other cities in the Hellenistic east. Although few new conclusions of overriding importance are advanced, several bear noting. For example, Pippidi establishes that the cult of the Egyptian deities Isis and Serapis was known by the second century B.C., while the common opinion considers them an importation of the period of Roman domination. The chapter on agricultural labor provides a refreshing discussion of the thesis that a free non-Greek peasantry worked the land for Greek landholders, in contrast to the prevailing views of Marxist east European historians that agriculture was slave-based. Similar views, more or less well argued if ultimately beyond proof with the current evidence, will reward the persistent reader. The opening chapter emphasizes the well-known limitations of literary sources for classical antiquity and stresses the importance of epigraphy, but it makes extravagant claims for this study. The second chapter furnishes a useful survey of the subject from the early nineteenth century to the present, with indications of current plans by Romanian scholars for systematic publication. In sum, the book is learned and informative but over-long and rather repetitious. It should be consulted by specialists rather than read through cover to cover.

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EDOUARD WILL *et al.* *Le monde grec et l'Orient*. Volume 2, *Le II<sup>e</sup> siècle et l'époque hellénistique*. (Peuples et civilisations: Histoire générale, volume 2.) Paris: Presses Universitaires de France. 1975. Pp. 678.

Volume 2 of *Le monde grec et l'Orient* covers the period from 404 B.C. to 188 B.C. and is a worthy successor to its predecessor, which was deservedly acclaimed in this journal as "the best one-volume history of fifth-century Greece there is (78 [1973]: 1029). The features that made volume 1 a pleasure to use mark the new work: clear and elegant writing and comprehensive bibliographical notes introducing each chapter. But while Edouard Will was solely responsible for volume 1, volume 2 is the result of his collaboration with Claude Mossé (who deals with the period between 404 and 336 B.C.) and Paul Goukowski (who writes about Alexander the Great). The book, therefore, provides a representative sample of the work of the very best contemporary French Hellenists.

True to the strong sociological interests of

French classical scholarship, two-thirds of the book are devoted to careful analysis of the social and cultural changes in Greece and the Near East during the fourth and third centuries B.C. Of this analysis the high point is Will's masterful account of Ptolemaic Egypt (pp. 461-80). Here, besides a lucid account of the current state of our knowledge of the Ptolemaic organization of Egypt and its dependencies, there is a convincing demonstration that the much-touted rational planning of the Egyptian economy by the third-century Ptolemaic bureaucracy is largely a modern myth.

It is perhaps churlish to complain when so much is good, but one major flaw mars this otherwise excellent work: it has no unity. Volume 2 actually consists of three excellent but essentially unrelated monographs. Mossé focuses her penetrating account of the fourth century on the crisis in Greek society caused by the disintegration of the communal organization of the polis. Goukowski's treatment of Alexander emphasizes the impact of a dynamic personality on his own times. Will brilliantly discusses those aspects of Hellenistic civilization he believes relevant to the concerns of the second half of the twentieth century. Despite the structural weakness, however, the high quality of its parts makes this book the best survey of the fourth and third centuries B.C. available in any language.

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K. D. WHITE. *Farm Equipment of the Roman World*. New York: Cambridge University Press. 1975. Pp. xvii, 258. \$35.00.

Once the property of pedants and gentlemen farmers intent on producing annotated editions of Cato's *De Agri Cultura* or erudite discourses on the pleasures of viticulture, studies of Roman farming have only recently begun to reflect historical rather than antiquarian interests. At first glance *Farm Equipment of the Roman World*, with its profusion of Latin terms and quotations (invariably translated), may seem retrogressive. Admittedly, White's procedure—classification by type and material—and his emphasis on literary descriptions of farm equipment are conventional, but his catalog is not so much a dictionary as a guidebook for further research on the technology and socioeconomic aspects of Roman agriculture.

What distinguishes *Farm Equipment of the Roman World* from its predecessors is White's command of the archeological, as well as the literary, evidence of Roman agricultural practices. Integration of the data, however, remains incomplete. From footnotes in excavation reports and the storage rooms

of local museums White has succeeded in rescuing the artifacts, disvalued by archeologists and collectors alike, but he cannot reconstruct their chronological and technological contexts. No one, in fact, has managed to overcome the inherent decontextualization of the objects, few of which have been recovered through stratigraphically controlled excavations. With improvements in recovery procedures classical archeologists may be able to eliminate this defect. But only when they begin to include in their reports scientific analyses of excavated agricultural artifacts (for example, neutron activation analysis of iron objects) will historians be able to place the items White catalogs in their proper technological and socioeconomic contexts.

This book is clearly an improvement on the companion volume, *Roman Agricultural Implements* (1967), which often compounded the problem through defective organization of the data, but it does not make use of available analytic techniques. Some historians, of course, may feel that the author has supplied more information on Roman baskets and basketmaking (part 2) or pots and pans (part 3) than they wish to know, but one must bear in mind that without "notes" of this sort a synthesis cannot be constructed. Nor would we have White's first-rate accounts of wine making (pp. 112ff.) and oil production (appendix A), processes in which many of the vessels he describes were regularly used.

Students of Roman agronomy will welcome this comprehensive guide to the artifacts. Those who prefer their syntheses plain should consult the author's *Roman Farming* (1970).

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KURT RAAFLAUB. *Dignitatis contentio: Studien zur Motivation und politischen Taktik im Bürgerkrieg zwischen Caesar und Pompeius*. (Vestigia: Beiträge zur alten Geschichte, volume 20.) Munich: C. H. Beck'sche Verlagsbuchhandlung. 1974. Pp. xvi, 358. DM 58.

This collection of specialized studies represents the revision of the author's dissertation submitted to the Universität Basel in 1970. It presents detailed analyses of the political maneuvering during 51–48 B.C.: the power struggle between Caesar and the *factio* grouped around Cato and the Marcelli. Regarding the *Kriegsschuldfrage*, Kurt Raaflaub's conclusions are not completely new. He maintains that civil war was not inevitable until at least December 50 B.C. and that only the "fanatische 'Ultras'" (Lentulus Crus, Metellus Scipio, Domitius Ahenobarbus, and possibly Bibulus) sought the war. Indeed, Caesar began the war (i.e., "Prä-

ventivangriff"), but both sides could claim right. After the political impasse of late 50–49 B.C., the civil war stemmed from personal motives.

In part 1 ("Zur Vorgeschichte des Ausbruchs des Bürgerkrieges"), Raaflaub focuses on the SCU of January 7, 49 B.C. and its debatable legitimacy. In order to scrutinize fully Caesar's charge of a *novum exemplum*, he offers a useful analysis—especially in light of the recent work of Ungern-Sternberg—of the application of SCU since its inception against C. Gracchus. Moreover, he indicates that in the political situation of January 49, it was not only a question of legitimacy, but also of power.

Part 2 ("Motive, Motivationsweise und Zielsetzung der Bürgerkriegsparteien") and part 3 ("Zur politischen Taktik der beiden Parteien im Bürgerkrieg") encounter the question of political propaganda in Caesar's *BC*, and perhaps here Raaflaub makes his major contribution. Since the previous scholarship on this subject cannot be properly appreciated from Raaflaub's narrative, J. H. Collin's bibliographical article in the "Cyclopean" Vogt *Festschrift* should first be read. Raaflaub stresses the priority that Caesar gives to personal motives in the *BC* (i.e., the *veteres inimicitiae*, *iniuriae inimicorum*, his *dignitas*, etc.), and he also discusses in detail the use and meaning of *libertas* as a political slogan in this contest. The discussion of the political tactics during the civil war examines the struggle for constitutional legitimacy, Caesar's *studium pacis*, and politics of *clementia* and *crudelitas*.

The concluding chapter, which places these studies in the context of late Republican history, and an excellent, analytical index of names, subjects, and sources greatly contribute to the book's usefulness. Raaflaub has necessarily been selective in citing the voluminous secondary literature, and he most frequently refers to the work of Meyer, Gelzer, Heuss, Strasburger, and especially Meier, whose influence is particularly noticeable. Certainly not all Raaflaub's arguments and conclusions will be accepted; however, subsequent scholars now have the responsibility either to accept them or to refute them.

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PHILIP TYLER. *The Persian Wars of the 3rd Century A.D. and Roman Imperial Monetary Policy, A.D. 253–68*. (Historia-Einzelschriften, number 23.) Wiesbaden: Franz Steiner. 1975. Pp. 56. DM 24.

Philip Tyler presents in tabular form the astonishing results of cupellation and chemical analysis of 402 antoniniani selected from the Riby (Lincolnshire) and Gibraltar hoards; antoniniani of A.D. 253–65 in the Gibraltar hoard have consistently

higher silver contents than those in the Riby hoard, even though the coins of both were struck at Rome. Tyler argues that this reflects manipulation of the currency of the East in response to military pressures. In effect, lighter and baser coins were reserved for the more backward West.

Whatever the significance of the remarkable compositional differences between coins of the two hoards, the author's thesis rests on two unacceptable assumptions: first, that the Gibraltar hoard was assembled in the East and therefore reflects Asian currency; and, second, that coins were ever struck at Rome expressly for Eastern circulation. Gibraltar may be the only Western hoard to contain a substantial number of eastern coins, but no hoard of Eastern provenance includes Western issues in such numbers. Furthermore, consignment of coins directly from Rome would have rendered existing Eastern mints superfluous.

The evidence is mystifying, its interpretation doubtful. The author correctly observes that more hoards of certain (and diverse) provenance must be subjected to rigorous analysis before the complexities of Roman monetary policy can be perceived.

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MALCOLM TODD. *The Northern Barbarians, 100 B.C.-A.D. 300*. London: Hutchinson University Library; distributed by Humanities Press, Atlantic Highlands, N.J. 1976. Pp. 232. \$13.00.

This judicious, informative book illuminates a culture unduly neglected by classicists, because it is not Greco-Roman, and by anthropologists, because it is too late. Of its seven chapters, the first defines "Germania," a convenient term for Germany, north Holland, Denmark, southern Norway and Sweden, and most of Poland and Czechoslovakia between 100 B.C. and A.D. 300. Its population tripled, from one to three million. Literary sources give its history, but Caesar, for example, exaggerated German savagery to justify his own campaigns, and he wrongly presented the Rhine as a cultural frontier: Germans either side of it show more likenesses than differences.

Instead of presenting static "culture provinces," Malcolm Todd divides Germania into thirteen archeological groupings, which are distinguished by pottery. He assigns these to tribes where the evidence warrants: for example, Quadi in Moravia; Chauci and Frisii on the North Sea coast; Semnones in Brandenburg; and Lombards and Saxons on the lower Elbe. The Marcomanni invaded Bohemia, and the Chatti, Cherusci, and Breucteri migrated into the Rhine-Weser area. Perhaps the Bastarnae were in Moldavia, the Cim-

bri, Teutones, and Angli in Jutland, the Heruli in the Danish islands, and the Goths and Burgundians in Pomerania.

The settlements of these groupings (Federsee, Fochteloo, Feddersen Wierde) consisted of log cabins, long houses, grids of corduroy streets, and *Herrenhäuser*. No mere seminomads, German farmers raised small cattle and horses, yoked oxen to their plows, grew grapes, and made elderberry wine. Their field systems faintly resemble Roman centuriation (fig. 19).

In technology, they were good metalworkers: Lysa Goar (Poland) is a vast industrial area with hundreds of smelting furnaces. They used the lathe, and they were great boat builders (Nydam, Hjortspring) and expert weavers. They borrowed their weaponry from the Celts and Romans: chain mail, longbows, parade helmets, embossed shields. They were poor at siegecraft, and they were well organized and efficient only under rare leaders (Maroboduus and Arminius).

They showed little Roman influence in their religious practices, for they had many localized deities. The Romans overemphasized lurid aspects; the Germans practiced human sacrifice, slit throats, and hanged, decapitated, strangled, and drowned their victims. They gradually began to use animals (dogs and horses) as substitutes. They were superstitious about divination: runes were originally magic signs. Christianity appeared only about 400.

These illiterates gave birth to medieval Europe. In them we see a primitive, remote society in contact with an outward-looking civilized power. They invaded the empire chiefly for provincial lodgment, to emulate, not to destroy; they were better equipped for life in the empire than Tacitus would have believed. Todd's argument is persuasive and highlighted by maps, figures, plates, and an enormous bibliography. This is a first-rate introduction to a complex, important subject.

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## MEDIEVAL

ROBERT S. LOPEZ. *The Commercial Revolution of the Middle Ages, 950-1350*. New York: Cambridge University Press. 1976. Pp. xi. 180. Cloth \$9.95, paper \$3.95.

Except for an updated bibliographical essay, this highly readable and imaginative little book is an unaltered reprint of the original edition of 1971. Robert S. Lopez surveys commerce, industry, and agriculture during these centuries under the theme of a commercial revolution that "created the in-

dispensable material and moral conditions for a thousand years of virtually uninterrupted growth" (p. vii). It was a necessary precursor of the industrial revolution of the eighteenth and nineteenth centuries, for most medieval industry was limited to domestic operations and did not attract large capital investments. Although Lopez admits that an agricultural surplus was a precondition of the commercial revolution, he finds that the changes operated essentially within the cities. The bourgeoisie were producing and importing an increasingly diverse assortment of commodities; but essentially these were to sell to other townsmen, for their rural clientele, although not negligible, was very limited. New products and commercial techniques barely affected the rural areas, and the most prosperous agricultural regions of Europe were the least urbanized. The best sections of the book describe commercial techniques, particularly those involving the introduction of more flexible credit operations than those of antiquity, the activities of Italian merchants outside their homeland, and technological change as a force of material progress.

This is a convenient survey for undergraduates, but I cannot recommend it without reservation. Lopez has tried to write both a general survey and an interpretive essay, but he has not been entirely successful with either undertaking. Even in areas of the author's particular strength, specific points must be questioned. He sees the commercial revolution as beginning with Italian involvement in trade with the Greeks and Muslims, yet he admits that the Venetians at first could offer only salt and glass in exchange for the luxury goods of the east. His view of a fundamental social opposition between *popolani* and a seigniorial aristocracy of magnates in the Italian cities is a gross distortion, for landowners played an important role even at Florence after the *popolani* came to dominate the city. The long distance trader and the wealthy industrialist were figures almost as exceptional in Italy as in the north, although one must agree with the author that their influence on economic change was very great.

More fundamentally, the commercial revolution is simply too narrow a theme for a treatment that is intended to be general, and Lopez has trouble throughout in adapting his discussions of agriculture and industry to accommodate a series of developments that he admits had little effect on either. His material is heavily slanted toward Italy, towns, and commerce, while his sections on agriculture, particularly in the High Middle Ages, are weak and outdated. Although the author admits that some deceleration occurred after 1250, he is almost alone among serious medieval economic historians in continuing to argue that the eco-

nomie growth of the High Middle Ages continued until the Black Death. Recent studies strongly suggest that medieval agriculture had reached its productive limits by the second half of the thirteenth century and that this was creating difficulties for the food supply in the overpopulated towns—a fact that in itself casts doubt on Lopez's postulate of the virtual independence of the cities from their agrarian hinterlands.

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SUSAN MOSHER STUARD, edited with an introduction by. *Women in Medieval Society*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1976. Pp. 219. Cloth \$15.00, paper \$4.95.

A tendency toward the diminishing of women's public roles, a lowering of ecclesiastical opinions about women, a withdrawal of women from guild activity, and the disappearance of women from urban and rural labor forces has been acknowledged so generally among historians of medieval and early modern Europe as to approach becoming a cliché. Using the hypothesis of the general deterioration of the position of women in the Late Middle Ages as her focus, Susan Mosher Stuard has chosen essays which examine its validity using a variety of perspectives including demography, quantitative analysis, anthropology, and legal and religious history. The position of women in regard to family and law, women and religion, and the status of women in relation to class and locale are some of the problems these essays examine, with each author implicitly or explicitly asking the questions: "why does the role of women seem more restricted in the Late Middle Ages than previously?"; "were women's roles as restricted as they seemed to be?"; "how did women perceive and respond to the particular attitudes and deprivations they experienced?"

The essays, written by David Herlihy, Emily Coleman, Heath Dillard, Brenda Bouton, Barbara Hanawalt, Sue Sheridan Walker, Stanley Chonicki, and Susan Mosher Stuard, are too detailed to be summarized adequately in the space allotted here. Their conclusions—based largely on impressive archival research—have notable impact when considered as a group and are bound to raise new questions and provoke more discussion and research.

In addition to suggesting new lines of scholarly investigation, the essays make a contribution to the teaching of women's history. The inclusion of six essays previously presented elsewhere can perhaps be justified because this volume demonstrates to students the importance of using different ap-

proaches to the same historical questions and the value of communication among the disciplines. The editor apparently had this particular audience in mind when she included a bibliography of the standard and most recent works in the area. Moreover, the bibliography, although limited, serves as a visible reminder of the far-reaching impact serious questions about sex roles have had on research on the medieval and early modern fields.

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WALTER ULLMANN. *Law and Politics in the Middle Ages: An Introduction to the Sources of Medieval Political Ideas*. (Sources of History: Studies in the Uses of Historical Evidence.) Ithaca: Cornell University Press. 1975. Pp. 320. \$15.00.

Walter Ullmann wrote *Law and Politics in the Middle Ages* as an "introduction to the sources of medieval political ideas." The book belongs to a series of similar essays, edited by G. R. Elton, that are intended as "a guide—for historians and students—to varied materials without which history could not be written." Ullmann contends that *Law and Politics* "is one of the first attempts to present the principal sources of medieval political ideas in an integrated and coherent manner."

Ullmann's masterly knowledge of the materials and his forceful argumentation provide us with a lively text. Those who have read his other works will already be familiar with many of his prominent themes, such as the theory of ascending and descending rulership. Yet the length of the book limits the author to only the highlights of his topic. He considers primarily the literary documents that relate to lay and ecclesiastical governance in Western Europe from the fall of the Roman Empire to the beginning of the Renaissance. He concentrates on Roman and canon law, and within these on the survival of the two traditions after the collapse of the western empire and their subsequent development after the rise of the school of Bologna in the late eleventh century.

Given his interests over the years, Ullmann's choices are not surprising. But one could hope for a more balanced treatment. The first two-thirds of *Law and Politics* (chapters 1 through 5) leave the impression that the history of political theory is the unfolding of Roman and canon law and that indigenous elements had little part except to acquiesce to those ancient traditions. Ullmann asserts, for example, that "next to the Bible, the Digest proved itself the most resilient, creative and influential formative organ to which the still half-barbaric West was subjected, and this in precisely those matters which constituted civilized living together" (p. 59). Ullmann first brings in Germanic

custom at the end of the chapter on Roman law, but only to illustrate how the various kings and emperors incorporated the classical Christian traditions into their policies. However, he devotes chapter 6 to the literary materials of non-Roman, secular governance. While this brief (thirty-page) treatment provides a useful discussion of the major types of documents that reflect royal policy and legal custom from the seventh through the thirteenth centuries, it does not have the depth or development of the first part of the book. The final two chapters shift to nonlegal literature that reflects governmental theory and practice. Chapter 7 considers such writers as Pope Leo I and John of Salisbury as it traces the development of doctrinal tracts through the twelfth century; chapter 8 reviews the transformation of political writing that followed the acceptance of Aristotelian philosophy in the early thirteenth century.

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LUCIEN MUSSET. *The Germanic Invasions: The Making of Europe, AD 400-600*. Translated by EDWARD and COLUMBA JAMES. University Park, Pa.: Pennsylvania State University Press. 1975. Pp. xiii, 287. \$16.50.

In 1965 Lucien Musset published his *Les invasions: Les vagues germaniques* as a part of the series "Nouvelle Clio," devoted to concise outlines of the state of relevant problems and current scholarly discussion (including citations of the most important recent work—one of the most valuable contributions of the series). French historians, as is well known, seem particularly able at this sort of thing. The present volume is a translation of Musset's book with the addition of various bibliographical items, and some minor rearrangement of the materials. The translation reads well and is, where I checked, regularly accurate, indeed, once or twice it accurately reproduces an error in the original.

The changes and additions, unfortunately, are not by Musset himself, who, to judge by the jacket, is alive and flourishing. I regret that this is not a thoroughgoing revision, for after ten years of additional scholarship, one would prefer a new edition by the author himself rather than a translation of the old with some patches added. More importantly, I wonder for whom the English version is intended. The book must be meant for the use of advanced students and practicing scholars, but I doubt that there are many such in this field who are unable to read French. On the other hand, less knowledgeable students will frequently be puzzled by unexplained names of persons and words denoting institutions; the book hardly seems appropriate for a beginner who can not, for example,



identify Amalaric. Presumably this version will have to suffice for scholars until the appearance of a real revised edition in any language.

STEWART IRVIN OOST  
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WALTHER KIENAST. *Deutschland und Frankreich in der Kaiserzeit (900–1270): Weltkaiser und Einzelkönige*. In three parts. (Monographien zur Geschichte des Mittelalters, volume 9.) Stuttgart: Anton Hiersemann. 1974–75. Pp. cxxvi, 785. DM 345.

In 1943, the year that marked the eleven hundredth anniversary of the Treaty of Verdun, the first edition of this work appeared in one slender volume. The author could not have chosen a more tragically appropriate time to treat the historic problem of Franco-German relations. Now, over two decades later, the one volume has grown to three in what the publisher describes as a completely reworked second edition. The statement is accurate in the following ways: an incomparably larger documentation accounts for a third of the new size; the sections on the tenth, eleventh, and first half of the twelfth centuries contain far greater detail than originally; the expanded study of the political ideology and legal theory of the empire makes the second volume a valuable addition; and seventeen appendices in place of two provide extensive investigations of minute collateral and supportive topics. The presentation of the period after 1150 has changed little, however, and the conclusion remains the same almost word for word.

His thesis is clear: why did the two political and cultural heirs of Carolingian Europe come to such mutually destructive and often implacable hostility? Enmeshed in this primary question is a second, which the author calls the fundamental problem of German history: why did Germany move from strength and unity in the tenth century to weakness and disunity in the thirteenth while France accomplished the reverse process within the same period?

Kienast's exposition brings one ultimately to feel that the Capetian kings, if not France, were at fault in this first Europe. Compared to the "Parisian shadow-kings" and their pretensions, the medieval empire was a laudable idea and an even more desirable fact. It sought not to master but to serve Europe by providing it with a religious and political unity that would have made war unnecessary. But France developed a concept of its own mission, a concept that was nourished by French zeal for crusading, ecclesiastical reform, and unprecedented cultural achievement, and came to maturity under Philip Augustus with the fiction that Capetian kings were the true successors to Charlemagne. In the process France cultivated an

arrogance, a haughtiness, a superiority that were destructive of political accommodation. The period thus ended with the triumph of particularism in the sovereign state, and the loss of the dream that a higher principle could give Europe unity, peace, and strength.

Writing always with verve—and often with passion—Kienast is a stark partisan of his thesis. Most French scholars and many third parties will differ greatly with his premises and conclusions. He ends his book in 1975 as he did in 1943, in quite different historical conditions, with the plea that the two nations use their great gifts and abilities to serve a greater whole, the new Europe.

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GIAN ANDRI BEZZOLA. *Die Mongolen in Abendländischer Sicht (1220–1270): Ein Beitrag zur Frage der Völkerbegegnungen*. Bern: Francke Verlag. 1974. Pp. 251. Sfr. 58.

The importance of Mongolian-European relations during the thirteenth and fourteenth centuries has long since been recognized by historians dealing with the wider issues of European history of the period and, particularly, with the crusades. This volume may now be added to a list of distinguished books and articles dealing with some aspects of the historical interaction between two fundamentally different civilizations.

The content of this excellent book neither belies nor exceeds the scope of its title. It is a thorough analysis of the portrait of the Mongols as it emerges from mainly Latin sources of the thirteenth century. Gian Bezzola does not depict the Mongols as they were, but rather as they appeared in the eyes of their European contemporaries. As can be expected, Western opinions on the identity, character, and role of the Mongols underwent considerable changes between their first appearance on the European horizon and their political fragmentation and complete demythologization.

The book is divided into four main parts: "Lebende und Realität (1220–1240)," "Die Schreckenszeit (1241–1244)," "Die ersten Vorstöße zu den Mongolen (1245–1255)," "Das Abendland zwischen Furcht und Erwartung (1248–1264)." A fifth part—it would have been more appropriate to call it an appendix—examines the place taken by the Mongols in Roger Bacon's conception of the world. In writing this book the author could have had no access to A. D. von den Brincken's fine *Nationes Christianorum Orientalium* (1973), which could have helped him in the preparation of the first chapter, and to Davide Bigalli's *I Tartari e l'Apocalisse* (1971), a detailed study of the role of the

Mongols in the eschatology of Roger Bacon and Adam Marsh. Otherwise the author's remarkable familiarity with primary and secondary sources covers almost the whole gamut of the relevant literature. Only the Russian secondary literature is untapped. Use of it would have permitted the identification (p. 113) of the Archbishop Peter of Russia who appeared at the Council of Lyon in 1245, with Peter Akarovich, *igumen* of a monastery in Kiev.

The erudition and the methodology of the author must command our respect. It is particularly fortunate that the original Latin text of the sources used is given in the footnotes. Praise is due also to the author's analyses of the sources of information used by the chroniclers. These permit a critical evaluation of the material provided and constitute a valuable contribution to our knowledge of medieval historiography.

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LYDD LAING. *The Archaeology of Late Celtic Britain and Ireland c. 400-1200 AD*. London: Methuen; distributed by Barnes and Noble, New York. 1975. Pp. xxvii, 451. Cloth \$35.00, paper \$16.00.

Lloyd Laing has chosen to extend into book form the lectures in Celtic archeology which he delivered to first-year undergraduates at Liverpool University. The volume, according to Laing, is not intended for specialists but is directed to "students for whom the minutiae can come later." The work consists of two main parts. In the first section are nine chapters devoted to Early Christian Celts, Southern Scotland, Northern Scotland, Wales and the Isle of Man, South-west England, Ireland, the Norse in Scotland, the Norse outside Scotland, and Celtic survival. The examination of the material remains in these chapters is carried out from a regional perspective. Scattered throughout these rather detailed descriptions of sites appear a few pages dealing with political and religious history, based usually upon considerably later and often controversial Celtic written sources. Part two is topically organized to cover technology and trade, subsistence equipment, personal adornment, art and craftsmanship, and the Church. The latter section will be more useful to undergraduates than the former, but by limiting this section to "Early Christian Celts" much of value is omitted. The volume also includes appendices on Celtic manuscript art and the chronology of some of the more important historical personages mentioned in the book. These appendices either should have been more comprehensive or else not included. The bibliography is good but the index sparse. In view of

the intended audience a glossary and a list of noteworthy archeological sites with some mention of their basic characteristics would have been helpful. In addition, although there are 151 text figures, many of which are maps, there is a great need for several comprehensive maps illustrating political, religious, and material remains in conjunction with each other.

The writing of a textbook such as this demands the synthesis of heaps of data and reams of interpretation. Faced with these problems, such works, usually covering rather long periods of time, must have some fundamental principle of organization. Thus, Laing has chosen to organize his synthesis of some eight centuries of the material remains of much of Britain and all of Ireland upon a linguistic hypothesis. He implies that those people who spoke one or another of the Celtic dialects (or languages) during this period share other fundamental characteristics—in this case a material culture—which gives them an identity or unity worthy of emphasis and makes them the proper subject of a synthesis. I must point out, however, that by the end of the twelfth century, and indeed much earlier, the various Celtic speakers in the several regions that Laing examined spoke mutually unintelligible dialects (or languages). Yet, during at least part of the 800 years under discussion, significant linguistic and cultural affinity existed between some of the Celtic peoples in Britain and those in Brittany. Laing, however, neglects this important bastion of Celtic speakers. The dubious value of the linguistic principle of organization for the study of archeology is given substantial impetus by the material remains he examines. This stuff is extremely diverse and hardly suggests a unitary culture. In short, *The Archaeology of Late Celtic Britain and Ireland c. 400-1200 A.D.* has about the same level of intellectual justification and cohesiveness as would a volume on the archeology of "Late Germanic" West-Central Europe and Scandinavia, 400-1200.

Despite its weaknesses, Laing's volume is a useful introduction to a substantial number of archeological sites in Britain and Ireland. The Viking chapters and the pages on pottery are the most useful.

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KENNETH HARRISON. *The Framework of Anglo-Saxon History to A.D. 900*. New York: Cambridge University Press. 1976. Pp. viii, 170. \$15.95.

This is a bright and quietly innovative volume that explores the various systems of reckoning time



used in Anglo-Saxon England down to the death of Alfred. A number of Kenneth Harrison's findings challenge current assumptions. He convincingly demonstrates, for example, that Bede and his successors began the calendar year on or about January 1 without exception, and that the credit for promoting the Dionysiac cycle in England and introducing the *Annus Domini* into dating clauses really belongs to Wilfrid. When Bede deserted the *Annus Mundi* along with the chronicle form and took up the Christian era to write a history, he employed a dating system already used in legal documents throughout England. Bede's contribution was to reduce the different modes of dating found in his sources to a uniform system, thereby placing the writing of history on a secure foundation for the first time since antiquity.

The study as a whole is notable for its detailed coverage and concreteness, with only a few curious lapses, such as having Attila besiege Rome in 452. It is unfortunate that Harrison's organization seems to obscure his message. The narrative of the book often appears as unsettled and circuitous as the calendars of the period. The seven chapters contain much unnecessary repetition, and I at least had a hard time throughout discerning where the author's argument was going. Part of the fault may be attributable to the spare, disjointed sources with which the author worked, causing him to have to flesh them out artificially; perhaps a more important contributory factor is that the individual chapters—based largely on previously published papers—have not been carefully integrated.

Harrison avoids easy generalizations. If the conclusions of his study are tentative and hypotheses more numerous than certainties, the volume is rich in penetrating observations and skeptical intelligence. The author (correctly, I think) rejects the traditional view that annalistic writing grew out of the tables used by the Church for calculating the date of Easter. He recognizes that no rigid set of criteria delimits annal from chronicle, and both from history. If there is a governing theme in Harrison's volume, it is the extraordinary openness of the Anglo-Saxons to innovation, a characteristic that—applied to chronology—allowed them to play a significant part in the development of historical writing.

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E. J. DOBSON. *The Origins of Ancrene Wisse*. New York: Oxford University Press. 1976. Pp. 441. \$37.50.

"Moderate enough I am, who ask for so little." These words by an anonymous author were writ-

ten to request prayers at the end of probably the oldest extant manuscript of *Ancrene Wisse*, the important Middle English guide for anchoresses composed in the early thirteenth century for a small circle of northern Herefordshire recluses. E. J. Dobson believes that the independent clause functions, in its West Midlands form, both as the interpretation of a Latin name and as an anagram indicating place, all of which reveals the medieval author's signature: Brian of Lingen.

The mention of hidden signatures, especially when they involve the arithmetic of anagrams, causes a flashing of amber lights. Mindful of this, Dobson saves the concealed name for the last (sixth) chapter of his book, and he presents it as the least of his reasons for thinking that Brian of Lingen wrote *Ancrene Wisse*. In fact, the persuasive chapters before the riddling begins really make this volume worth its fearful price. In these Dobson argues that the anonymous author was a secular canon of the Victorine abbey of Wigmore; that he wrote between 1215 and 1222; that his work had special connections with Limebrook Priory; and that the sisters of Limebrook remembered Ralph of Lingen (d. 1190) as their first founder, and his son Brian (d. ca. 1235) as *canonicus saecularis monasterii de wigmore*. Many other things also help to clarify the early history of the manuscript. By the time Dobson gets around to the signature, he is already convinced that Brian of Lingen was the right kind of man, at the right time and place, to have written the work. His cautious treatment of the signature brings the book to a most effective, even suspenseful, conclusion. Six appendixes examine such matters as the history of the Lingen family. In all, the study is worthy of the scholar to whom it is dedicated, the late J. R. R. Tolkien, himself a distinguished student of *Ancrene Wisse*.

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DAVID C. DOUGLAS. *The Norman Fate, 1100-1154*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xv, 258. \$22.50.

Like its predecessor *The Norman Achievement* (*AHR*, 75, [1970]: 1433-34), *The Norman Fate* is a thoughtful exercise in comparative history; as was its predecessor, it is a synthesis of historical scholarship rather than a contribution to knowledge, aimed at the intelligent reader of history. Like Gaul, the work is divided into three parts. The first is a survey of the political history of the Anglo-Norman state (see, on this topic, C. Warren Hollister, "Normandy, France, and the Anglo-Norman Regnum," *Speculum*, 51 [Apr. 1976]: 202-42) and of the Sicily of Roger the Great. The second section deals with the impact of the Normans upon Europe,

discussing society, government, church history, and intellectual history. The book concludes with an admirably imaginative discussion of the role of the southern Normans in the Second Crusade, concentrating on the personalities and policies of Bohemund of Antioch, Tancred of Antioch, and Roger the Great. The wisdom and cautious use of sources that characterize David C. Douglas' scholarship are apparent throughout, although some readers will feel that sometimes he claims too much for the Normans' contribution to European history, and that sometimes he presents as Norman what is really European.

I have some cavils with *The Norman Fate*, all so small as to be niggling. I find it rather jarring and old-fashioned to write of Henry II that "his own temper could more properly be described as French rather than Norman." At times Douglas omits reference to recent and relevant scholarship; for example, when shrewdly discussing the Norman impact upon secular government, surely he should have alluded to that deceptively simple distillation of a lifetime of learning and reflection, Joseph Strayer's *On the Medieval Origins of the Modern State*. Such reservations as these should not be taken as detracting from the best extant synthesis of the place of the Normans in the history of Europe in the first half of the twelfth century.

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ELEANOR SEARLE. *Lordship and Community: Battle Abbey and Its Banlieu, 1066-1538*. (Studies and Texts, number 26.) Toronto: Pontifical Institute of Mediaeval Studies. 1974. Pp. 479. \$20.00.

In October 1066, when Duke William of Normandy was about to commit himself to what was to be called the Battle of Hastings, he swore that if successful he would found a monastery on the battlefield and would dedicate the site to God. William kept his word. This volume is a very detailed and scholarly chronicle of that abbey, Battle Abbey, and its external relationships with king and community during the next five centuries until its destruction by Henry VIII in 1538.

There are six parts and seventeen appendixes to the volume. Perhaps an indication of its contents is the easiest way to make evident the importance of the volume. Part 1 discusses the Conqueror's *Eigenkloster* with its exemptions, manors, and purchases, detailing the settlement of the land and of the town and its constitution. It is concluded by a discussion of the problems connected with the domination of patrons and servants. Part 2 shows the abbey during the thirteenth century with its land markets, burgesses, types of revenues, and purchasing patterns. It also includes a detailed

discussion of the abbey's customary tenants. Parts 3 and 4 show us the abbot as *Judex* in his liberty and its relation with such varied contemporary developments as the question of *quo warranto*. The discussion of the abbot as *dominus* brings up arable production, convertible husbandry, the labor force, and dispersal of the demesne. Parts 5 and 6 trace the community at the end of the Middle Ages (1303-1538) and the decline of its lordship, investigating such items as the court as the community, civil pleas, and law days.

One of the important contributions of the volume for the medievalist is the fact that it portrays the external economic relations (which often include judicial and administrative developments) for some five centuries, thereby showing monastic adaptations through this long period. Anyone interested in the various economic changes in land use will be able to see in a single abbey how such changes were actually effected, and will also be able to control somewhat the generalizations that may at times be too easily made. There is a wealth of material in the copious footnotes and appendixes that makes this fine volume even more helpful to the researcher.

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THEODORE EVERGATES. *Feudal Society in the Bailliage of Troyes under the Counts of Champagne, 1152-1284*. Baltimore: Johns Hopkins University Press. 1976. Pp. xii, 273. \$14.00.

Theodore Evergates has written a valuable but limited book on the social structure of the southern region of Champagne. In it he first examines the obligations of the peasantry and the impact of community franchises, issued from the late twelfth century. Then he devotes two chapters to the aristocracy, including a quantitative analysis of several surveys of comital fiefholders compiled between 1172 and 1275—a unique series of documents. Finally he compares his findings with those of studies on other regions of France, thus placing the work in the context of the best recent French historiography.

The picture of social hierarchy that emerges from the book is marked by precision and subtlety. With the aid of quantitative measures and a careful analysis of the language of the documents, Evergates goes beyond the traditional categorization of the medieval population as either peasant or aristocratic, either rural or urban. He depicts a society where such differences are best viewed along continuous scales.

But the book suffers from injudicious limitations of geography and subject. In terms of social development, the *bailliage* was not an entity that can be

successfully isolated from the rest of Champagne. The author's decision to deal only with this restricted area seems to have been dictated by the availability of quantifiable evidence, a problem that might have been overcome by a more imaginative methodology. (Evergates applies quantitative methods only to sources that were cast in quantitative terms to begin with. Fossier's work, one example among many, shows that such methods may be useful in the analysis of charter evidence as well.) The limitation of subject arises from an undue concentration on status, with only the most cursory treatment of economic conditions or the local and regional organization of power. As a result, status receives a rather formalistic and unsatisfactory definition. Its import is not clarified through an analysis of the interaction of status groups in the political and economic spheres. Can we really describe a society merely in terms of status? Evergates answers the questions that he poses for himself in the introduction, but the inadequacy of the questions limits the value of his conclusions.

The major contribution of the book is the development of copious data on a regional aristocracy in the form of both quantitative indexes and genealogical details. (The latter constitute an excellent, lengthy appendix.) A more comprehensive and conceptually sounder treatment of a medieval society, based on the author's use of the data and his admirable knowledge of the history of Champagne, would be welcome.

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EMMANUEL LE ROY LADURIE. *Montaillou, village occitan de 1294 à 1324*. (Bibliothèque des Histoires.) Paris: Gallimard. 1975. Pp. 642.

This is a stunning book and to read it is to experience something like Keats's emotions upon first looking into Chapman's Homer. Although technically a work of local history (and very local at that), *Montaillou, village occitan* does more to enrich our understanding of life in the Middle Ages than any book since Marc Bloch's *Société féodale*.

At the opening of the fourteenth century Montaillou was a small Pyrenean town of some 200–250 souls situated at 4300 feet in what is now the Haute Ariège. Its inhabitants enjoyed the modest but roughly equal standard of living provided by a mountainous economy. In religion, some were Catholics and others Cathars, and it is this latter circumstance that makes Emmanuel Le Roy Ladurie's study possible. For in 1320 Jacques Fournier, bishop of Pamiers and the future Benedict

XII, brought the Inquisition to Montaillou, and it is largely from the resulting documentation—six volumes in total—that Le Roy Ladurie has fashioned this masterpiece.

Historians have traditionally used inquisitorial records only to study the Inquisition itself or, more recently, the nature of heresy. Le Roy Ladurie, on the other hand, has used them to find out about people, the society they created, and the world in which they lived—this last being defined both in terms of physical reality and in terms of the thoughts and beliefs with which that reality was invested. Like others in the *Annales* school, Le Roy Ladurie brings to his task a strikingly sophisticated knowledge of the modern social sciences (notably ethnography), but at the same time he never allows theoretical constructs to come between the reader and the people being studied.

As a result, the range and nuances of Le Roy Ladurie's conclusions defy easy summation; the book itself must be read in its entirety—and then reread for the sheer enjoyment of it. The reader thus comes to grips not only with these villagers, but also with such things as the central role played by the nuclear family—the *domus*, *maison*, or *ostal*—in their lives. Clearly, the *ostal*, not the individual, provided the animating force of this little society. From jokes to sexuality, little escapes Le Roy Ladurie's attention, and he uses each monographic detail patiently to build an all-encompassing picture of Montaillou that deepens and transforms our understanding of medieval life. Like the novels of Kazantzakis, this book stands as a monument to the perseverance of the human spirit. Thus, even though few readers will find people like the Clergue family wholly sympathetic, this reviewer was nonetheless moved to discover that in 1970 that name was still listed in the Montaillou telephone book.

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MARJORIE CHIBNALL, editor and translator. *The Ecclesiastical History of Orderic Vitalis*. Volume 5, *Books IX and X*. (Oxford Medieval Texts.) New York: Oxford University Press. 1975. Pp. xix, 413. \$48.60.

Orderic Vitalis described his *Historia Ecclesiastica* as a little book on ecclesiastical matters. In reality the work is long, rambling, and indispensable to scholars of Anglo-Norman history, both as a detailed political record and as a window into Orderic's world of churchmen and nobles. For generations, historians have been well served by Auguste Le Prévost's monumental edition (Paris, 1838–55), on the basis of which Thomas Forester published a sloppy English translation in the 1850s. Since 1969, successive volumes have been

appearing of Marjorie Chibnall's splendid new edition and translation of Orderic's masterpiece.

The present volume comprises the ninth and tenth of the thirteen books into which Orderic divided his history (only books 1 and 2 and 11 through 13 remain to be done). In previous volumes, Chibnall's edition has included significant improvements on Le Prévost's. But in books 9 and 10, edited from an autograph manuscript in the *Bibliothèque Nationale*, her reading does not depart appreciably from that of her predecessor. The importance of her contribution lies primarily in her felicitous translation (a tremendous improvement on Forester's), and in her introduction and footnotes. Although Le Prévost's annotations were superb for their time, Chibnall's bring to bear on Orderic's account the scholarship of the past century.

Orderic's book 9 is a history of the First Crusade, from Clermont to Ascalon, drawn mostly from Baudry of Bourgueil's *Historia Ierosolimitana*. Since Baudry was not himself an eyewitness, Orderic's account is largely third-hand. Its factual value is slight, but it marks an important stage in the elevation of the Crusaders' deeds into the realm of heroic legend. Book 10 brings us to earth again—to Normandy and England during the closing years of William Rufus's reign and the opening year of Henry I's. But in the end Orderic redirects his attention to the Levant, where once again distance blurs fact into romance.

Chibnall's footnotes stalk Orderic throughout his wanderings, correcting his errors, untangling his chronology, identifying persons and places, supplementing his data with that of other sources and with apt bibliographical references. Scholars will doubtless disagree with her translation at points, and perhaps with a few of her comments. She casts unnecessary doubt, for example, on Orderic's statement that William of Warenne opposed Henry I in 1101 and was banished from England (p. 308, 1n.; compare *Regesta Regum Anglo-Normannorum* [vol. 2, no. 621] and *Liber Monasterii de Hyda*, p. 305). But all medievalists will admire the intelligence and erudition that Chibnall has brought to this significant scholarly enterprise.

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JAMES E. BIECHLER. *The Religious Language of Nicholas of Cusa*. (American Academy of Religion Dissertation Series, number 8.) Missoula, Montana: Scholars Press and American Academy of Religion. 1975. Pp. viii, 240.

Although much work has already been done on Nicholas of Cusa, the basic conclusion of this dis-

sertation supplements both the older body of German scholarship, which saw Cusanus as a precursor of "modernity," and Paul Sigmund's excellent recent study, which presents Cusanus' political theory as "medieval" in its sources and content. James E. Biechler's study sees Nicholas' central insights as religious and rightly tries to understand them as arising from, and to some extent resolving, the personal crisis of 1437—when Cusanus broke with the council of Basel—and the subsequent turmoil of a life combining ecclesiastical administration and a yearning for withdrawal. Biechler's analysis of religious themes is sensitive, and his scholarship careful and substantial.

There are, however, some problems with this dissertation. The author asserts his goal to be a demonstration of the "power" of Cusanus' religious language, by which he seems to mean its relation to Cusanus' personal and historical context. Yet his actual discussions of context—i.e. of Nicholas' life, of other fifteenth-century thinkers, and of broader social and cultural trends—are extremely thin and often based on out-of-date secondary accounts. Furthermore, he has no consistent method for demonstrating how a particular image refers to social reality and veils this deficiency in much confusing jargon. Biechler's study should either have been substantially revised to make its presentation of the relation of language to society more sophisticated, or it should have claimed and attempted less.

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PAUL STRAIT. *Cologne in the Twelfth Century*. (Florida State University Book.) Gainesville: University Presses of Florida. 1974. Pp. 156. \$10.00.

KURT-ULRICH JÄSCHKE. *Burgenbau und Landesverteidigung um 900: Überlegungen zu Beispielen aus Deutschland, Frankreich und England*. (Vorträge und Forschungen, special edition, number 16.) Sigmaringen: Jan Thorbecke Verlag. 1975. Pp. 136.

Kurt-Ulrich Jäschke's book represents a healthy tendency among German historians to transcend national boundaries and undertake comparative studies. His thesis is that fortifications and the defense of the countryside were first systematically integrated only in the post-Carolingian period. Although his scholarship is truly massive, the thesis is neither altogether novel nor well developed. The "Überlegungen" of the subtitle is an unfortunately accurate choice of words. Jäschke admits to the preliminary nature of his remarks and to gaps in his research, especially in the realm of archeology. He says nothing about France and virtually nothing



ing about Flanders. The discussion focuses principally on the failure of the Carolingians to establish comprehensive *Landesverteidigung* against the invaders of the ninth century (forty-seven pages out of 121), the defensive *burhs* of Alfred and Edward, and the so-called "Burgenordnung" of Henry the Fowler. Even here Jäschke's "considerations" are curiously imbalanced. He devotes enormous attention to chronological questions, whereas he rather cavalierly treats a far more central problem, the composition of the *familia* of the monastery of Hersfeld, one of our few sources of knowledge about the "Burgenordnung." The prose is dense and badly punctuated, and the arguments are almost invariably difficult to follow. Specialists may cull much useful material from these pages, but as a general work the book is most disappointing.

Paul Strait's monograph on Cologne reflects a rising interest among Anglo-American historians in premodern German history. Books and articles on the early modern period have been tumbling from the presses of late, and the German Middle Ages are now beginning to attract some attention. Strait's book is a good contribution to this field. It is also a solid and careful study in medieval urban history. He is well acquainted with the pertinent scholarship, and his findings concur with those of recent scholars who reject the long-regnant Marxist view of towns as created by merchants to be "islands in a feudal sea," and who stress that towns grew out of, rather than in stark opposition to, medieval society. Strait's argument is that Cologne developed peacefully in the twelfth century, not on the basis of a *coniuratio* (for which no evidence exists) or in conflict with the archbishops. Indeed, communal government evolved in the absence of strong archiepiscopal control, with at least the tacit consent of the archbishops, and in part out of the existing administration. The urban court, whose *scabini* were in the service of the archbishops and worked closely with the prominent men of the city, provided the core for the growth of self-government. Parishes supplied the crucial basis for governance at a more local level. *Scabini* and parish magistrates soon organized in powerful guilds. The merchants' guild, by comparison, played no discernible role in the achievement of autonomy and, in fact, disappeared from the records by mid-century. Similarly, burghers were not necessarily merchants, but rather those who held property in Cologne and hence included *ministeriales* and clerics. While the patriciate is very difficult to study before 1150, its origins and composition were also diverse. Few of its members in the twelfth century can be shown to have engaged in commerce; wealth and office-holding were demonstrably far more important qualifications. By

the early thirteenth century the patriciate was becoming a politically restrictive elite and would soon enter into open conflict with the archbishops; but neither of these tendencies characterized the twelfth century. Strait thus warns us to avoid not only perceiving medieval towns as essentially different from the rest of medieval society, but also imposing on the twelfth century conclusions that may be true only of the thirteenth. The book is not handsomely produced and has some typographical errors, but otherwise it is a useful and stimulating study.

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DELNO C. WEST, editor. *Joachim of Fiore in Christian Thought: Essays on the Influence of the Calabrian Prophet*. In two volumes. New York: Burt Franklin. 1975. Pp. xxiv, 227; 231-631. \$28.50 the set.

These two volumes contain photographic reprints of twenty essays concerning the twelfth-century Calabrian abbot Joachim of Fiore. The editor hopes "to present a proper background to encourage and inspire new studies about Joachim and his influence in Western thought" (p. i). After a brief introduction, the editor lists the genuine works of Joachim, followed by a bibliography of Joachim studies that have appeared since 1954. From the mass of literature relating to Joachim, four topics have been chosen to illustrate his influence in Western thought.

The first group of articles attempts to fix the canon and writings attributed to him: G. La Piana's "Joachim of Flora: A Critical Survey" (pp. 3-28); M. Bloomfield's "Joachim of Flora: A Critical Survey of his Canon, Teachings, Sources, Biography, and Influence" (pp. 29-91); and B. Hirsch-Reich's "Eine neue 'oeuvre de synthèse' über Joachim von Fiore" (p. 93-103).

The second group of articles concerns the spread of Joachim's canon and teachings: M. Bloomfield's and M. Reeves' "The Penetration of Joachimism into Northern Europe" (pp. 107-28); F. Russo's "Gioachinismo e Francescanesimo" (pp. 129-41); and E. R. Daniel's "A Re-Examination of the Origins of Franciscan Joachimism" (pp. 143-48).

The third group of articles deals with the influence of Joachim on new religious orders: M. Reeves' "The Abbot Joachim's Disciples and the Cistercian Order" (pp. 151-67); A. Messini's "Profetismo e profezie ritmiche italiane d'ispirazione Gioachimito Francescana nei secoli XIII, XIV, e XV" (pp. 169-207); and M. Reeves' "The Abbot Joachim and the Society of Jesus" (pp. 209-27).

The fourth and longest group of articles, comprising the entire second volume, illustrates Joachim's influence on church and state: H. Haupt's "Zur Geschichte des Joachimismus" (pp. 231-91); H. Grundmann's "Frederico II e Gioachimo da Fiore" (pp. 293-99); E. R. Daniel's "Apocalyptic Conversion: The Joachite Alternative to the Crusades" (pp. 301-28); H. Grundmann's "Dante und Joachim von Fiore, zu Paradiso x-xii" (pp. 329-75); E. Buonaiuti's "Gioacchino da Fiore, San Bonaventura, San Tommaso" (pp. 377-85); E. Benz's "Joachim-Studien III. Thomas von Aquin und Joachim de Fiore" (pp. 387-451); B. McGinn's "The Abbot and the Doctors: Scholastic Reactions to the Radical Eschatology of Joachim of Fiore" (pp. 453-71); B. Hirsch-Reich's "Joachim von Fiore und das Judentum" (pp. 473-510); M. Reeves' "Joachimist Influences on the Idea of a Last World Emperor" (pp. 511-58); R. Kestenberg-Gladstein's "The 'Third Reich': A Fifteenth Century Polemic Against Joachimism, and Its Background" (pp. 559-609); and C. Frazer's "Gonçalo Martínez de Medina, the *Jerónimos*, and the *Devotio Moderna*" (pp. 611-31).

No collection of essays such as this could hope to satisfy the interests and needs of every reader. Some might quibble over which essays should be included or omitted, but by and large the editor offers a fairly representative cross section of the best Joachimite literature. The bibliographic essays are especially useful in providing the groundwork for further research into the thought and influence of this enigmatic prophet of the twelfth century. One must ask, however, whether the convenience afforded by this collection of essays outweighs the high cost of the two volumes.

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DENIS A. ZAKYTHINOS. *Le Despotat grec de Morée*. Volume 1, *Histoire politique*; volume 2, *Vie et institutions*. Edited by CHRYSSA MALTÉZOU. Revised edition. London: Variorum. 1975. Pp. iii, 394, 429.

When the first volume of Denis A. Zakythinos' study of the Greek despotate of Morea (the volume that treated political history) was published in 1932 it was widely hailed as the definitive study of the unique administrative division of the post-Crusader Byzantine Empire. The same evaluation holds true today. By meticulously analyzing all the available evidence, Greek and foreign, both written and archeological, Zakythinos created a careful picture of the development of the Greek state in the Peloponnesus at the expense of the petty Frankish states and the recalcitrant native landholding archons. His analysis of the success of

Greek policy in Morea—that it stemmed from the practice of giving local governors viceregal authority (making them "despots"), while assuring the loyalty of the autonomous Byzantine "vassal state" by restricting its leadership to members of the imperial family—is as generally accepted today as the chronological framework he so carefully established. This is not to say that our knowledge of the history of southern Greece has not improved in forty years (see the important contributions of Bon, Laurent, Loenertz, and Barker, for instance), but that scholarship has tended to complement and expand Zakythinos' presentation and to correct it only in detail. What the "augmented edition" at hand has done is to integrate the results of the last two generations of scholarly work on later Byzantine history into Zakythinos' still standard work. The mechanism used to effect this updating is quite successful. Where the photomechanical reproduction of the original allows, simple changes, revisions, and additions are inserted into the original text and footnotes and are marked either by a bold type or square brackets. Larger emendations, which range from additional bibliographic citations to analysis of conflicting opinions, are collected in the fifty-four pages of page-keyed supplementary notes in the back of the book. An appended listing of relevant bibliographic items, published since the first edition, is extremely useful, if not quite complete.

Volume 2 of Zakythinos' *Le Despotat grec de Morée*, originally published twenty-one years after volume 1, is a topical analysis of the material treated in a chronological-political framework in the first volume: population, social structure, economy and finances, government, religious affairs, and intellectual life. It is less successful than volume 1, primarily because insufficient data exist for a penetrating investigation of these aspects of the Moreote experience. And, in fact, what can be said has for the most part already been covered in passing, as it were, in volume 1. Two clear exceptions to this statement are the chapters on the Church and on intellectual developments, which go considerably beyond what is said in the earlier volume. It should be noted that the supplementary material added to update the reprint of volume 2 is much less complete than in volume 1.

In general, Chryssa Maltézou's revision of this monumental study goes far beyond mere mechanical updating. It summarizes forty years of scholarship and is thus in its own right a contribution to the advancement of our understanding of the history of Greece in the later Middle Ages. As such Maltézou's "revised and augmented" edition must now be accounted the standard history of the Morea in the Palaeologan period. The new indexes

and the excellent foldout map of the Peloponnesus by Anna Avramea in volume 2 add significantly to the usefulness of the new edition.

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KAROL MODZELEWSKI. *Organizacja gospodarcza państwa piastowskiego: X-XIII wiek*. [Economic Organization of the Piast State: X-XIII Centuries]. Wrocław: Polska Akademia Nauk, Instytut Historii Kultury Materialnej. 1975. Pp. 296.

During the past three decades the question of the economy of medieval Poland has been a central problem in Polish historiography. The present work by a younger scholar represents a successful effort to provide a synthetic picture of the economic organization of the Piast state prior to 1300. It is firmly based on both the sources and the very significant monographic research of such leading *doyens* as K. Buczek, G. Labuda, H. Samsonowicz, and A. Gieysztor.

The first chapter of the book analyzes the role of princely courts in the economic organization of the monarchy; the author's thesis is that such centers constituted the focus of royal economic concerns. A second chapter presents an analysis of a contrary view, namely, that those *castra* under royal administrative control served as the vital center of Piast economy. The long concluding chapter provides a detailed description of the structure of princely legal rights and activities as these bear upon economic matters. Two general conclusions emerge from this careful and reliable presentation. The first is that ducal and princely rights were indeed the means by which the early Piast monarchy exploited the economic resources of the kingdom. The second is that the nature of Polish society in this period was rather different from that of feudal Western Europe. While there was a feudal society in Poland, it was centralized, as one can see from the effectiveness of royal authority particularly in the economic sphere.

Karol Modzelewski correctly points out that the thirteenth century was a crucial period during which the disintegration of the monarchy and the introduction of important Western influences created a political and economic crisis of great proportions. His study marks an important contribution in understanding the extent of this change.

PAUL W. KNOLL  
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PAUL W. KNOLL. *The Rise of the Polish Monarchy: Piast Poland in East Central Europe, 1320-1370*. Chicago:

University of Chicago Press. 1972. Pp. x, 276. \$11.00.

This excellent monograph portrays fifty years of Polish history, the period during which the partitioned, feuding principalities of the Piast dynasty made the transition toward a strong centralized state under crowned monarchs. After a lapse of over two centuries the Piast dynasty re-established its rights to sacral kingship, and the last representative of the dynasty, Casimir III, placed Poland on the diplomatic map of Europe on equal standing with Bohemia (for a while the center of the empire) and Hungary. Knoll skillfully presents the diplomatic and military efforts of Władysław Łokietek to assert control over the discordant Piast princes and to regain territories lost temporarily to foreign domination. New dynastic links binding the Piasts with the ruling families of Lithuania and Hungary created for his son Casimir the possibility of maneuvering against the Teutonic Knights. At the same time, old dynastic links of the Piasts with the Rurik family, as well as past Polish control of Halicz, and a chain of favorable circumstances created for Casimir the opportunity to expand eastward.

Knoll gathered his evidence from sources insufficiently analyzed in the past. Especially impressive is his tracing of diplomatic history and of military events leading to the acquisition of Halicz. But his occasional generalizations do not follow from his own evidence; Knoll clearly establishes Casimir's dynastic rights and claims for the area (p. 128), but he draws the unwarranted conclusion that Casimir invaded and conquered most of Ruthenia (pp. 141-42). Similarly, one should not conclude that the "eastern boundaries of Poland had been extended some 150 miles" (p. 142) if the realm of Ruthenia was in fact inherited as a unit and was not incorporated into Poland (compare Casimir's title *Rex Poloniae et Dominus Russiae*, referred to on pages 141 and 171). Even ecclesiastically, Halicz remained under its own Orthodox and Catholic metropolitans.

Outside the main scope of this study is Knoll's belief that Mieszko received baptism from Bohemia (p. 1). But this could not have been the case; adult baptism had to be administered by a bishop, and Prague did not have a bishop until 973. One may also note that the Tatars were not turned back by the Poles or the Hungarians (p. 44); they left Central Europe on their own volition. Moreover, it seems premature to place Slovakia and Pest on a map of East Central Europe as of 1320, while omitting Croatia (p. 52).

Knoll's study is a valuable addition to the meager number of titles in English dealing with the



medieval history of East Central Europe. A paperback edition of this work would well serve the young medievalists who want to expand their vision of European history beyond the Rankean limits.

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WILLIAM URBAN. *The Baltic Crusade*. DeKalb: Northern Illinois University Press. 1975. Pp. xii, 296. \$17.50.

William Urban addresses himself to a public that has lacked an English-language introduction to the early history of the Baltic states, of Livonia in particular. His presentation spans the period from the beginnings of the Christian missions in the late twelfth century to the merger of the Order of the Brethren of the Sword and the Teutonic Knights and the definite establishment of their rule at the end of the thirteenth century. His story is told in the traditional fashion, with emphasis on diplomatic, political, and military events as reported in medieval sources and recounted in much of the historical literature of the past. Political interests of acting personages and internal power struggles in Livonia play the major role. The author hardly discusses social, economic, cultural, and demographic issues, gives little interpretation, and keeps strictly to the topic as stated in the title, namely the "crusade" of German and Danish knights within the framework of the general crusading movement of the age. He chiefly describes the actions of popes, nuncios, missionaries, bishops, kings, nobles, and crusading knights, and of native pagans—Letts, Semgallians, Samogitians, Estonians, Lithuanians—who were fighting, or making alliances with, one another or the knights. The author shows no prejudices common among Russian historians (for example, in connection with the battle on Lake Peipus) or Baltic nationalists (such as Olins), but he presents the story, though rather simplified, in a factual manner. Urban does not pass over the brutalities that all sides committed, or such honest convictions as they may have held. He writes with sympathy for all parties involved in the struggle, and he shows that, while a viable solution was reached by the end of the thirteenth century, the foundation was also laid for further struggles, both during the late Middle Ages and in modern times. Footnotes are few, but useful maps are included.

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ROBERT BROWNING. *Byzantium and Bulgaria: A Comparative Study across the Early Medieval Frontier*.

Berkeley and Los Angeles: University of California Press. 1975. Pp. 232. \$11.50.

This slender volume of 183 pages, not counting the highly selective critical bibliography, notes, chronology, and index, weighs just half as much as the author's *Justinian and Theodora*, one of the most magnificent books on Byzantine history. Any book on Bulgaria or Byzantium, even such a peculiar one as Arnott's *Byzantines*, is welcome, but one on both is a luxury.

Robert Browning's erudition is impressive, especially in the use of Bulgarian material, as is his frequent admission of lack of information. Perhaps this lack accounts for his use of the old-fashioned expression "Dark Ages." "Early Medieval" in this case signifies the ninth and tenth centuries, the "high-water mark" (Hussey) of the Byzantine "Macedonian" period. In Bulgarian history, it lasted from official Christianization to the end of the first empire in Macedonia.

A relatively long introduction on the Balkans and on the chaotic "ethnic mosaic" resulting from invasions, including the arrival of Slavs and Bulgars, summarizes early chapters of Obolensky's *Byzantine Commonwealth*. Chapters follow on foreign and military relations; land use and ownership; cities, or lack of them, except notably Pliska and Preslav (Browning omits mention of Preslav glass); industry and trade; political structure; religion (the Bulgarian conversion is the high point of both that chapter and the book); culture, mainly literature; and everyday life (the shortest and least satisfying chapter).

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P. A. YANNPOULOS. *La société profane dans l'empire byzantin des vi<sup>e</sup>, viii<sup>e</sup>, et ix<sup>e</sup> siècles*. (Université de Louvain. Recueil de travaux d'histoire et de philologie, sixth series, number 6.) Louvain: Publications Universitaires de Louvain. 1975. Pp. xlvii, 331. 560 fr.

This work presents an analysis of the elements which constituted the society of the Byzantine empire. The qualification "profane" simply means that the clergy is not included, a regrettable omission because the clergy—especially the monks—was an important constituent of that society. Nevertheless, the analysis is remarkable.

On the basis of Byzantine law, scholars may place the people of the empire into three broad categories: the free, the servile, and those whose freedom was curtailed by certain incapacities. This classification provided P. A. Yannopoulos with the broad outline of his book, but its essence

lies in the analysis which it offers of the levels of Byzantine society which actually obtained. Particularly interesting in this respect is the section devoted to the free persons.

Byzantine society was very religious; it was also very secular. Nobility of birth, position, and wealth were held in high esteem. These values, especially wealth, determined the level in society to which one belonged.

Yannopoulos has based his book strictly on the sources, so much so that he has refrained from making any assertions for which he was not able to find any direct evidence. On this basis he has questioned the soundness of several generally accepted views, and this is, of course, as it should be. Nevertheless, one may ask whether the strict literalness of his approach may not have unduly curtailed his imagination. He has produced evidence, for instance, to the effect that according to the Byzantines it was God who chose and at times deposed their emperors. But he rejects the generally accepted view that the Byzantines considered their emperors to be the representatives of God on earth because he found no direct evidence to that effect. Maybe. But it is not an unreasonable inference drawn from what is specifically known.

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#### MODERN EUROPE

M. L. RIGHINI BONELLI and WILLIAM R. SHEA, editors.  
*Reason, Experiment, and Mysticism in the Scientific Revolution*. New York: Science History Publications. 1975. Pp. vi, 320. \$20.00.

This collection of papers from an international conference held in Capri, Italy, in April 1974 confronts two major issues in the Scientific Revolution: the role of mysticism in the formation of modern science and the nature and historical origins of Galileo's natural philosophy. It settles neither, but it provides a stimulating, current introduction to both. Because of limitations of space, I will confine my comments to the former and merely commend the papers of Righini, Gingerich, Drake, Crombie, and Costabel to the reader's attention.

R. S. Westfall provides the volume's most radical reinterpretation with his argument that Newton was an alchemist (at least through the mid-1690s), that his concept of universal gravitation derived primarily from his alchemical active principles, and that the *Principia* (1687) should perhaps be viewed as merely an interruption of his primary labor, alchemy. I cannot accept these claims without additional argument and evidence, largely because Westfall's interpretation goes beyond his

evidence. For example, I cannot see how Newton's early definitions of force, impetus, and inertia from *De gravitatione* (c. 1669) are anything but mechanical and thus cannot comprehend how they present "a view of nature that any alchemist would recognize" (p. 218). Westfall also ignores evidence contrary to his interpretation: after all, Hooke in 1679-80 did repeatedly suggest to Newton that planetary motions could be explained by an attraction. Moreover, he never explains, other than chronologically, the relation of Newton's alchemy to his Neoplatonism, which both preceded and outlived his alchemical interests and which first provided him with his concepts of active and passive principles.

Westfall's paper is nonetheless valuable, for by carefully describing Newton's intense alchemical activity—his readings, experiments, and writings (well over one million words)—he demonstrates that alchemy was undoubtedly one of Newton's central interests and cannot simply be ignored or wished away. Here I found M. B. Hall's comments, based on her intimate familiarity with seventeenth-century chemistry, most cogent. She suggests that behind all this alchemical activity, Newton is not to be viewed as an "alchemist *tout pur*," but rather as rationally attempting to understand its underlying principles. Moreover, she properly urges that Newton's alchemy be studied conceptually within the context both of his sources and of his own parallel, chemical investigations, a task Westfall has only undertaken descriptively.

The volume concludes with a trenchant but sometimes overly harsh paper by Paolo Rossi, who admonishes that the role of mysticism in the emergence of modern science must be kept in its proper perspective and not be overemphasized at the expense of the truly revolutionary features of the Scientific Revolution.

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THOMAS NIPPERDEY. *Reformation, Revolution, Utopie: Studien zum 16. Jahrhundert*. Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 146. DM 12.80.

This book is a collection of four essays that first appeared in the 1960s. According to Thomas Nipperdey, each is meant as a contribution to the problem of defining the character of the early modern age and as an attempt to bring intellectual and social history closer together. The two modern concepts of revolution and utopia serve as focal points of his concerns. Each essay has copious notes, a bibliography, and an afterword that carries the discussion up to 1974.

The West Germans' concern about their eastern

neighbors can be clearly seen in these essays. "The Reformation as a Problem in Marxist Historical Science" (1967) and "The Peasant War as an Historical Problem" (1966) explicate DDR and Soviet interpretations of the German Reformation and contrast Marxist with Western viewpoints. Although Nipperdey rejects the monocausal Marxist explanation in favor of the less rigid, more pluralistic Western interpretations, he also advocates dialogue between the two sides. Indeed, he maintains that Western historians sometimes overemphasize the irrational elements in history and do not sufficiently interrelate the religious, political, and economic causes of the Reformation.

In "Theology and Revolution in Thomas M ntzer" (1963) Nipperdey contends that the starting point of M ntzer's "theology of the cross" was much closer to Luther than is sometimes asserted. Further, M ntzer's revolutionary social ideas can be derived directly from his own theology. According to Nipperdey, M ntzer's thought shows a number of modern elements such as a radical subjectivity, a critique of alienation, and a revolutionary desire to change the world, but these elements cannot be explained through an economic interpretation.

"Thomas More's 'Utopia' and the Beginnings of the Modern Age" (1966), in contrast to a number of interpretations, emphasizes the realistic and modern elements of More's book. Especially important is the idea that the world is not something given or sanctioned by religion; rather, it can be formed and changed by men. More argues that reason and institutions mold men, so that men are changed when institutions change.

In summary, these essays do not provide a unified interpretation of the early sixteenth century, but they offer interesting theses about some of the specifically modern elements found in that era. They also clarify some of the controversies between Marxist and Western historians over the beginnings of the modern age.

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WILLIAM R. ESTEP. *The Anabaptist Story*. Revised edition. Grand Rapids, Mich.: William B. Eerdmans Publishing Company. 1975. Pp. viii, 250. \$3.95.

J. HOWARD KAUFFMAN and LELAND HARDER. *Anabaptists Four Centuries Later: A Profile of Five Mennonite and Brethren in Christ Denominations*. Scottdale, Pa.: Herald Press. 1975. Pp. 399. \$9.95.

William R. Estep's book is a revised edition, incorporating the advances made in Anabaptist historiography since the original edition appeared in 1963. I agree with him that the essence of Anabap-

tism is its historical view of the Bible, its Christocentric hermeneutic (which sees Jesus as Messiah establishing the disciple-church as the kingdom of God in social and political confrontation with the kingdoms of this world), and its fruition in the Anabaptist community. Praise was educed, even from Anabaptism's enemies, for its high standards of personal morality, but it was condemned by them for its social ethic, which espoused religious liberty and rejection of a state church, the magistracy, the oath, and war.

The J. Howard Kauffman-Leland Harder volume is an analytical study of computerized responses, by a stratified 3,591-member sample, to a comprehensive questionnaire designed to measure the faith and life of 200,000 North American Mennonites by the standards of their sixteenth-century forbears (Amish and Hutterites are not included in the study). The results reveal a remarkable perpetuation of Anabaptist qualities: two-kingdom theology, biblicism, and commitment to the disciple-community. Tabulated scores on participation in the corporate life of the church almost universally are higher than those of other religious groups for which comparable studies are available. In matters of personal morality (integrity, family life, simple living, temperance) Mennonites "still exercise a rigorous discipline . . . refusing to accept as valid . . . many behaviors much more easily accepted or tolerated by general Christendom."

Social ethics scores are lower, however (seventy percent in agreement with Anabaptist norms, fifteen percent undecided, and fifteen percent disagreeing), owing largely to an element of fundamentalist sentiment, high scores of which are in consistent negative correlation to pacifist commitment, racial tolerance, social witness, and Bible knowledge. Fundamentalism, which made its impact early in the century when it was mistakenly seen as an ally of the Anabaptist kingdom in confrontation with the world, is now in decline, with the resurgence of Anabaptist studies, which bring a fresh perspective enabling mid-century Mennonites to "see more clearly . . . that they belong neither in the modernist nor fundamentalist camps, but have a satisfactory biblicism and evangelicalism of their own with its unique Anabaptist heritage."

Although the Anabaptist-minded authors find "little comfort" in the too numerous *halbbl uferisch* revelations of the study, sanguinary evidence, on the other hand, is sufficient for concluding that whatever else may be said of quadricentennial Mennonites, more than the sect-cycle theory is required to explain them. "There is an undeniable continuity through four centuries of history. . . . With a few noted exceptions, doctrinal adherence to the Anabaptist vision has moved twelfth gener-

ation Anabaptists to a position that stands against the stream of society on most of the indicators. . . . They have not returned to the 'churchly type' of morals and ethics."

This volume bears every mark of empirical research that is carefully planned and scrupulously executed. The interpretation of findings is well done.

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OWEN CHADWICK. *The Secularization of the European Mind in the Nineteenth Century*. (The Gifford Lectures in the University of Edinburgh for 1973-74.) New York: Cambridge University Press. 1976. Pp. 286. \$18.95.

It is usually assumed that the modern age has been marked by the reduction of religion to a marginal factor in social life. In the accepted version, religion once sanctified constitutional structure and organized human activity; today it is an optional interest of private individuals. Most surveys refer to this phenomenon under the rubric of secularization. In the Gifford lectures of 1973-74, the Regius professor of modern history at Cambridge essays an inquiry into the extent and the meaning of the process covered by the term, which itself only became current in the third quarter of the last century and first engaged the interest of sociologists. Owen Chadwick insists that it should become the concern of historians who are more alert to the ambivalence of the complexity of continuity and change. That he is becomes evident from the definition of the term that he offers: "the relation (whatever that is, which can only be known by historical inquiry) in which modern European civilization and society stands to the Christian elements of its past and the continuing Christian elements of its present."

The tentative nature of this statement does not preclude a very sophisticated examination of the aspects of the phenomenon in the major countries of the West. In the author's view, part of the impulse for the mutation in modern culture derived from Christianity, with its emphasis on the supremacy of conscience and the validity of human hope. But once the liberal state evolved, it was logically secular since it had to respect the views of all its citizens, whether they accepted or rejected religion. When the press became free it entered the realm of argument on moral and religious issues, sharpening and magnifying the debate and carrying it to the farthest village. Marxism became the most powerful philosophy of secularization in the industrializing world, and it appealed to many workers who saw the churches on the side of the political conservatives. Chadwick argues that evidence is lacking that industrialization naturally

led to atheism or that the working class was drawn to secularism by science, Darwinism, or Marx; the working class came to scorn churches as bastions of an unjust order, but it might still respect religion. He cautions against placing undue emphasis on the popular explosion that destroyed the archiepiscopal palace in Paris in 1830 (incorrectly locating it at Saint-Germain and ignoring the wider attacks on the mission crosses) or the more favorable popular attitudes in the same city in 1848; circumstances and accident played a major role.

As the century progressed, divisiveness on religion spread. Michelet, a religious man, attacked Christianity as inimical to justice and freedom. The growth of historical consciousness called into question the Biblical foundations of Christianity. Anticlericalism flared with the coming of universal suffrage in countries where Catholicism was feared for its influence on the electorate. Traditional moral attitudes were challenged by a *Realpolitik* that defended wars for the creation of new nations. Darwinism became a symbol of the grip of iron necessity to those who made it a secular form of predestination. And an articulate minority argued that morality could flourish apart from religion.

These propositions are presented with great urbanity, in a style that is attractive and occasionally whimsical. The limitations of intellectual history are clearly stated, especially the difficulty of defining when the views of the vocal few affect the whole society. Nor does Chadwick believe that we can know to what degree the process represented the perennial task of adjusting the religious understanding of the world to new human knowledge. The survival of the stone cross atop the secularized Pantheon may remind us of the complexity of replacing the old with the new.

This volume will invigorate but will not win unqualified assent. It would be an ideal stimulant for seminar discussion.

JOSEPH N. MOODY  
Boston College

JUDITH H. SHKLAR. *Freedom and Independence: A Study of the Political Ideas of Hegel's Phenomenology of Mind*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1976. Pp. xv, 216. \$14.95.

Written specifically for "students of political theory, both undergraduate and graduate, who have found the *Phenomenology of Mind* incomprehensible," this is certainly the best English-language introduction to Hegel's most interesting and original book. It is far more than just a guidebook to the *Phenomenology*. Judith N. Shklar, in a number of essays skillfully woven together, judiciously distinguishes between the "living and the dead" in Hegel's philosophy and politics, sub-



jecting Hegel's own conclusions to a sober and somewhat disenchanted reappraisal. Accepting Hegel's critique of philosophical individualism, she agrees that the road to truth involves a critical awareness of the course of history and culture. She is more skeptical of the finality of Hegel's act of remembering, which, however, she considers high art involving imaginative selection, rather than an irrefutable consequence of philosophical reasoning.

Without disregarding entirely the structure of the *Phenomenology*, to which she in fact devotes her entire first, rather weak, chapter, Shklar is more attracted to the brilliance with which Hegel portrays historical models and thought structures and their inevitable dissolution than to the specific content of "Absolute Knowledge" in which Hegel's own argument culminates. While all of her interpretations of Hegel's vignettes are very good, her discussion of the encounter of noble and bourgeois in early modern Europe as well as her portrayal of the plight of moral man—a recurring theme—is excellent. Aligning herself with the mainstream of postwar scholarship on Hegel in West Germany and North America, as represented by leading articles in *Hegel-Studien* or by Walter Kaufmann's *Hegel*, the author bases her interpretation more on Hegel's romantic nostalgia for Periclean Athens than on his explicit commitment to modernity. Passing over quickly, at times rejecting as unsuccessful if not disingenuous, Hegel's philosophical ingestion of modern German Christian theology, she sees his despair with modern political life, thought, and culture rather than his affirmation of "what is" as crucial.

A superb introduction to Hegel for novices and a fascinating essay in political theory in its own right, this book does not purport to be a study in the history of ideas. Concerned with rethinking "Hegel's thoughts and our own," Shklar has little interest in the specific context of the *Phenomenology* within German idealism as a whole. What little she does have to say about Kant and Fichte—Schelling's name is not even mentioned—relates entirely to Hegel's polemic against their ideas in the *Phenomenology*. Perhaps the author is right in thinking that interpretive and historical expositions are mutually exclusive; still the total absence of the latter limits considerably the usefulness of this study for scholars.

FRANZ NAUEN  
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LOUIS L. SNYDER. *Varieties of Nationalism: A Comparative Study*. Hinsdale, Ill.: Dryden Press. 1976. Pp. ix, 326.

This thoughtful and well-structured book, written in a pungent style, is dedicated to the memory of

Hans Kohn and Carlton J. H. Hayes, pioneer scholars of nationalism and guides and friends of the author. It will fill a definite need. The study is designed for the college student and the general reader rather than for the specialized scholar. Louis Snyder has succeeded well in sketching the history of nationalism, bringing it up to date, and adding to it a comparative and critical dimension. Snyder's insights are the result of a lifetime of preoccupation with this phenomenon.

In the first five chapters the author furnishes a theoretical framework, summarizing the work of earlier and of leading contemporary scholars. He stresses the need for a multidisciplinary approach to the study of nationalism, and he treats questions of semantics, classification, and the problem of national character. After tracing nationalism from its appearance in Europe to World War II, he discusses in chapters 10 through 19 the new nationalism, examples of the fragmentation of nationalism in Europe (the mininationalism of the Basques, Scots, Welsh, Flemings, and Walloons, Croatians, etc.), the rise of macronationalism (various "pan" movements such as pan-Slavism, pan-Arabism, etc.), and the relationship between tribalism and nationalism in Africa. Snyder focuses on the similarities between American and Soviet Russian nationalism, their "impulse to Messianism," rather than dwelling on their differences. He is clearly skeptical of the "panaceas," world, regional, and continental, that have been offered as likely to overcome the obstacles to collaboration and permanent peace between opposing nationalisms. In the battle of the "isms," nationalism emerges triumphant, and both socialism and communism are successful only when identified with nationalism. The author reluctantly reaches the conclusion that nationalism which he considers "divisive, chaotic, anarchic," will persist into the foreseeable future. After earlier stressing the positive aspects of nationalism, he ends on this pessimistic note.

A stimulating and timely book that extends the reader's horizon to new continents and concentrates on the new nation-building in the postwar era, *Varieties of Nationalism* should prove a valuable companion reader to more traditional texts in European and world history or international relations.

ALFRED D. LOW  
Marquette University

MICHAEL SILAGI. *Henry George und Europa: Zur Entstehungsgeschichte der europäischen Bodenreformbewegungen*. Munich: Etana. 1973. Pp. viii, 193. DM 25.

This book is a reprint of the author's doctoral dissertation at the University of Munich. It is based on a thorough study of printed sources and a

number of unpublished letters by, to, and about the American social reformer Henry George.

A brief introductory chapter discusses George's theory that a single land tax on rent is the key to removing social injustice. The remainder of the book treats George's theoretical and practical influence on Britain, Germany, Hungary, and Denmark, where, ironically, his ideas had more of an impact than in his own country. Earlier works have treated George's influence on Britain, particularly on the ideas of the non-Marxist left, but his influence on such areas as Hungary is less familiar. The author's primary service is to bring this formerly scattered material together.

While George's theories influenced a wide variety of thinkers, his practical impact was minimal. Only the pre-World War I German colony of Kiaochow received a single land tax on the order of the Georgian model. As the author points out, after World War I many optimistic, rationally based reform movements lost their driving power. Such was the case with George's land-tax proposal.

DONALD E. THOMAS, JR.  
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THOMAS A. BAILEY and PAUL B. RYAN. *The Lusitania Disaster: An Episode in Modern Warfare and Diplomacy*. New York: Free Press. 1975. Pp. xv, 383. \$10.95.

In their detailed and lively new account, Thomas A. Bailey and Captain Paul B. Ryan, USN (Ret.), put to rest many myths concerning the sinking of the *Lusitania* on May 7, 1915, and evaluate the subsequent crisis in broad perspective. They contend that, given the twisting of international law by both naval belligerents, the Germans had a good legal case in attacking the *Lusitania* without warning in their declared submarine war zone. They argue that the explosion following the detonation of the torpedo by about thirty seconds came from rupturing boilers and steam lines, and not from munitions stored in the forward cargo hold. They insist that the *Lusitania* "almost certainly would not have been sunk if Captain Turner had honored any one of four or five explicit instructions" from the Admiralty, including maintaining a high speed and zigzagging in dangerous waters (p. 184). They also show that the official British inquiry conducted by Lord Mersey and the 1918 liability suit against the Cunard Company in New York covered up the substantial negligence of the Admiralty, and especially Captain Turner.

Sensational charges advanced by Colin Simpson's *Lusitania* (1972) furnished the provocation and many points of inquiry for the Bailey-Ryan study. The authors take great pains to establish

that, contrary to Simpson's contentions, guns were not mounted on the *Lusitania*; the Cunard liner did not carry munitions other than those listed in the final manifest, and no munitions exploded; there was no organized body of Canadian troops aboard the ship; the German submarine U-20 did not set out to trap the *Lusitania*; and Winston Churchill and Admiralty officials did not deliberately expose the large liner to U-boat attack in order to bring America into the war against Germany. Although their arguments are generally persuasive, Bailey and Ryan devote too much space to refuting Simpson, whose careless research and conspiratorial reasoning do not warrant such attention.

*The Lusitania Disaster* continued Bailey's long-standing crusade against Wilsonian diplomacy by attacking the policy of strict accountability as legally unjustified and politically unwise. Moreover, the authors show how Wilson expanded his claims when challenged by the *Lusitania* incident to insist for the first time that American citizens had the right of safe passage "on belligerent ships carrying munitions into proclaimed danger zones" (p. 63). They do not, however, resolve the contradiction between their repeated criticisms of American diplomacy and their recognition that Wilson won significant concessions and that for Germany the *Lusitania* incident "was a naval victory worse than a defeat" (p. 340).

Notwithstanding these strictures and a few rank puns (such as that referring to the debate about munitions on the liner as "an explosive subject" [p. 319]), Bailey and Ryan have produced a valuable study that stands as the most authoritative work to date on the destruction of the *Lusitania*.

SAMUEL F. WELLS, JR.  
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ARYEH L. UNGER. *The Totalitarian Party: Party and People in Nazi Germany and Soviet Russia*. (International Studies, published for the Centre for International Studies, London School of Economics and Political Science.) New York: Cambridge University Press. 1974. Pp. viii, 286. \$13.95.

ROGER MANVELL and HEINRICH FRAENKEL. *The Hundred Days to Hitler*. New York: St. Martin's Press. 1974. Pp. 245. \$8.95.

These two works focus on the problem of totalitarian power: one on its seizure, the other on its retention. The Manvell-Fraenkel work traces Hitler's coming to power in 1933. Aryeh L. Unger's work assesses the role of the Bolshevik and Nazi parties in the retention of power in those two societies. To Manvell and Fraenkel it was Hitler's



genius, his "unique personality" alone that held a disparate party together as he ascended the last rungs of the ladder to power. Unger studies the role of the party once power has been won. He emphasizes that the party is most crucial in maintaining control of a people, and the continuation of Soviet society during the twenty-three years since Stalin's death is cited as further testimony to the role of the party rather than the leader.

Manvell and Fraenkel survey the 107 days from the fall (December 7, 1932) of Gregor Strasser, one of Hitler's left-hand men, until the passage of the Enabling Act (March 27, 1933). It is as thorough a survey as one can get from wading in up to the ankles in source materials. There are no new historical discoveries from obscure archives. As a matter of fact, hardly any documentary sources were used by the authors. But it is a useful and readable summary of Hitler's last steps to the Reichschancellery, packaged in about one hundred pages. The balance of the volume, about ninety pages, is divided between the "Prelude," 1930-32, and the "Aftermath," from 1933 to mid-1934. These latter sections are understandably thin.

A major drawback to reading this work is the inclusion of a bundle of letters, quoted verbatim, page after page, in one instance running over twenty pages with scarcely a break (pp. 54-78). It almost makes one suspect history by stapler. Another annoyance, to me, is the clustering of all footnotes at the end of the work, which I consider appeasement of the printer at the expense of the reader. Aside from these annoyances, Manvell and Fraenkel have offered an answer to one of the murkiest problems of modern history—that it was Hitler and not the Nazi party who seduced the German people into giving him power in 1933.

Unger, on the other hand, deals with party functions rather than leaders. He concentrates on the parties in their relationships to the people, rather than their functions in government. This is a refreshingly different approach.

Unlike Manvell and Fraenkel, Unger has plunged up to his ear lobes into all kinds of material, published, unpublished, and archival. With meticulous detail he describes how both the Bolshevik and Nazi parties intruded into the lives of everyone in the country—into their work, their homes, their families, even their leisure—through "Agitators" or "Block Leaders" or "Comrades' Courts." He also makes a thought-provoking comparison of the paternalism both parties felt toward the people. Despite ideological differences, both parties praised the masses on the one hand, yet held them in contempt on the other. Both parties wanted mass support, yet both were afraid of letting the masses get hold of the torch of revolution

that they, the parties, were supposed to keep aflame.

It is unfortunate that Unger's work is very tedious and difficult to read. He has extracted and presented so much evidence from so many sources that the work is confoundingly difficult to digest. Nevertheless, its approach to the role of parties endows it with the quality of a trailblazer work for future students of totalitarianism.

It is difficult for Americans to understand the human side of totalitarian power. While these two works do little to unravel the riddle, they show us we have far to go.

DAVID B. STENZEL  
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Stanislaus*

SIDNEY ASTER. 1939: *The Making of the Second World War*. London: Andre Deutsch. 1973. Pp. 455. £3.95.

When in 1970 the British government made public its files on the origins of the Second World War, Sidney Aster stood at the head of the queue in Chancery Lane. To supplement the official record, Aster drew for this study on the private papers and reminiscences of nearly all the important policy makers then active in London. He obtained access to Neville Chamberlain's letters (temporarily available in Viscount Templewood's papers) and benefited from a friendship with Viorel Virgil Tilea, Romanian minister to Great Britain in 1939. The result of Aster's labors nevertheless amounts to something less than a multinational re-evaluation of the events leading to the outbreak of hostilities. Despite the title of the book, Aster focuses almost exclusively on British policy in the half-year between Hitler's march into Prague and Whitehall's reluctant declaration of war.

The author fashions his diplomatic history on a traditional last; the impact of the Nazi menace on British domestic politics and the clash of ideologies on a European scale concern him only peripherally. He gives us simply an account of high policy, as reflected in cabinet meetings, Foreign Office minutes, telegrams sent, and dispatches received. Yet if the outlines of this story have been sketched before on the basis of published documents, never have the details been rendered with such authority, clarity, and devastating impact.

Neville Chamberlain, Aster demonstrates, always distrusted Hitler after the Nazi takeover of the rump of Czechoslovakia. Still, he did not give up his search for a "reasonable" formula that would satisfy Hitler's appetites in East Central Europe and thereby save the peace. Hence Britain's guarantees of Poland, Romania, and Greece

served a purely diplomatic purpose. Britain could not offer effective military assistance in Eastern Europe, nor did it intend to do so. It sought through guarantees not to challenge Hitler, but to encourage him to negotiate for an orderly revision of frontiers, and to afford Poland and Romania time to make concessions without war. Aster exculpates Chamberlain and his associates from the charge that they considered defaulting on commitments to Poland if attempts to arrange a peaceful transfer of Danzig and the Corridor broke down. British leaders intended to keep their word to make abrogation of Polish independence a *casus belli*. But in case of war they planned nothing more than a blockade and a propaganda campaign, along with a holding action on the western front.

This clarification of British policy toward Poland goes far to explain why Anglo-French talks with the Soviet Union broke down in August 1939. Aster does not ignore the mutual suspicions, intelligence failures, and errors in timing that contributed to the collapse of these negotiations, but, he makes clear, differences in strategic outlook were fundamental. Drawing on Soviet monographs and documentary collections, he contends that the Russians sought an effective military alliance to deter Germany from aggression (though Stalin, to be sure, had flirted with the idea of a pact with Hitler as early as 1934). The British, on the contrary, did not want—until it was too late—an arrangement that would provide for mutual assistance in case of military confrontation in the east; they aimed only to head off a Russo-German rapprochement.

Not until war broke out did Chamberlain and his advisers begin to understand that their intricate maneuvers to satisfy Hitler had never stood a chance of success. “*Nothing wd. have made any difference*,” Sir Alexander Cadogan minuted in retrospect, “—except complete surrender to Herr Hitler’s demands.” Why did Chamberlain and others in Whitehall not realize this earlier? Aster fails to address this question, but the answer may lie more in the province of psychology than in that of old-style diplomatic history.

STEPHEN A. SCHUKER  
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HANSON W. BALDWIN. *The Crucial Years, 1939–1941: The World at War*. New York: Harper and Row. 1976. Pp. vi, 499. \$20.00.

*The Crucial Years* constitutes the first of a planned two-volume history of the Second World War. Its author, widely known as a veteran reporter on military subjects for the *New York Times*, has already written vivid commentaries (*Great Mistakes of the War*, *Battles Lost and Won*) and many articles on

the topic. His new book is vintage Baldwin. The bench marks of his style—rapidly paced narrative, a sense of drama, outspoken admiration and criticism—are all here. But if one had hoped that Baldwin would fuse his long experience (he is now seventy-two) with new insights gained from archival research or from mastery of recent analytical literature, one will be disappointed. Baldwin has not ventured much beyond description. For example, on one of the great decisions of the 1940–41 period, Hitler’s determination to invade Russia rather than to continue against England or Gibraltar, Baldwin offers almost nothing. The negotiations with Franco and the role played by Admiral Canaris (who is not mentioned) are completely glossed over. This kind of gap is apparent throughout: the author, while citing many standard Anglo-American studies, shows scant familiarity with the more scholarly European monographs; he is not interested in probing complex causal relationships or in presenting a complete picture of the war. Baldwin therefore has much less for the serious student than someone like John Lukacs, whose recent *The Last European War* also attempts an overview of 1939–41. Lukacs has produced, much more comprehensively than Baldwin, a sensitive analysis of the European war in not only military, but also social and political terms. One may disagree with Lukacs but one cannot disregard him.

Scholarship aside, I still find problems in Baldwin’s occasionally quixotic choice of emphases, sometimes clearly made for the sake of the dramatic (like the *Graf Spee* or *Bismarck* episodes) and sometimes possibly for convenience (some sections have already appeared in the *New York Times*). Why, for example, should we have more detailed coverage for the conquest of Hong Kong than we do for the fall of Poland? In short, Baldwin’s new book will not, as the dust jacket affirms, stand out as the authoritative account of the crisis of the Second World War; but for the general public it will provide a sound and rapidly moving account of the early campaigns.

ARTHUR L. FUNK  
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JOHN LUKACS. *The Last European War: September 1939/December 1941*. Garden City, N.Y.: Anchor Press. 1976. Pp. x, 562. \$15.00.

This book is not just another narrative of the early years of World War II. Like John Lukacs’ earlier writings, it is done with flair and, at times, brilliance; moreover, it is a highly personal work in both tone and content. Readers may sometimes be surprised, irritated, or even outraged, but they will not be bored. In the end, almost anyone should

emerge both better informed and provoked into rethinking (though not necessarily abandoning) some of the ideas he or she had taken for granted.

Lukacs has chosen to confine his account to the strictly European phase of the war, before the entry of non-European powers made it a global affair. His decision to end the story midway through the war creates no problems in part 1, which moves chronologically through the years 1938–41. After all, we know the sequel and can fill it in for ourselves. The effect is a little more awkward in part 2, entitled “The Main Movements,” which constitutes the heart of the work. In these chapters Lukacs sets out to analyze and synthesize a whole series of important but difficult and amorphous topics: the effects of the war on people’s lives, on political movements, on interstate relations, and on national attitudes, values, and beliefs. The problem here is that social, institutional, and attitudinal changes did not come to a full stop at the end of 1941. Suspending the analysis at that point leaves one, therefore, with a sense of an experiment interrupted halfway. Even so, this section is a rich mine of information and ideas on many aspects of wartime life that have been neglected.

Some readers will doubtless consider the book “revisionist.” A more accurate label, perhaps, would be “iconoclastic.” Lukacs delights in puncturing what he regards as myths, in challenging received ideas, and in proposing provocative new interpretations. The tone is sometimes contentious (or, as the British would say, “bloody-minded”); the text and the lengthy footnotes are peppered with epithets that historians rarely use: nonsense, idiotic, simplistic, adolescent, blast of guff, ugly mug. The author’s fundamental stance is conservative; he disparages the left as usually wrong, the intellectuals as “self-centered and silly,” the “liberal mind” as taking for granted that “Nazism was much more criminal than Communism.” If there are any heroes in the story, they would be Churchill and de Gaulle; Stalin, on the other hand, comes off especially badly. Hitler is not rehabilitated in any moral sense, but he is portrayed as a misunderstood figure whose sanity was above suspicion and whose political and military talents were impressive. The earliest and most effective resistance to nazism, according to Lukacs, was on the right; the workers were more susceptible to Nazi blandishments (even to the point of welcoming the German troops when they marched into Paris in 1940). The Germans (except for the Jews) fared far better under Hitler (and were more free) than the Russians under Stalin. A constantly recurring theme is the central importance of national character or national “habits of mind”; ideology is rated (in Gaullist fashion) as far less

important than national interest. Lukacs also insists on “the influence of mind over matter”; turning Marx on his head, he describes the material institutions of society as merely the superstructure of what men think and believe.

Possibly the most striking bit of iconoclasm in the book is Lukacs’ treatment of Hitler’s Jewish policy. Hitler’s “principal purpose” in life was to “solve the Jewish problem”—that is, to expel the Jews from Europe. Pearl Harbor made that solution impossible and impelled him to turn to extermination. But the “final solution” was such an unspeakable horror that it killed off all possibility of anti-Semitism, at least for the short-term future. If the Eastern European Jews had survived, a wave of anti-Semitism would probably have engulfed both Europe and America in the postwar years. Hitler’s decision therefore meant, in fact, that he had “lost his war against the Jews.”

Few recent books have impelled me to such a combination of admiration and dissent; few have led me to disfigure the margins of my copy with so many question marks and exclamation points. Such a book, I believe, justifies the author’s purpose: it deserves to be widely read, seriously considered, and vigorously debated.

GORDON WRIGHT  
Stanford University

WILLIAM A. LESSA. *Drake’s Island of Thieves: Ethnological Sleuthing*. Foreword by FRED EGGAN. Honolulu: University Press of Hawaii. 1975. Pp. xviii, 289. \$12.00.

On September 30, 1579, Sir Francis Drake’s *Golden Hind* dropped anchor at a Pacific island that he called the “Island of Thieves.” Since then, the identity of that island has been the subject of some speculation. The qualifying adjective is used advisedly because James Burney, a prominent geographer and author, early in the nineteenth century identified the island as one of the Palau islands. This was confirmed in recent times by two knowledgeable maritime historians, J. C. Beaglehole and Samuel Eliot Morison, although other scholars have continued to disagree. William A. Lessa’s purpose in his book is not so much to prove that the location of the “Island of Thieves” was in the Palaus as it is to lead us, step by step, through the reasoning process by which he arrived at his conclusions. He explains that the reasons for his efforts are, first, “to examine how far anthropology can throw light on the enigma of his [Drake’s] western Pacific landfall” and, second, “to demonstrate the reliability of the ethnohistorical method” (p. 10).

The author begins by examining the documentary sources, which are not only fragmentary

but secondary as well. Using these data he estimates the population of the island, basing his assumption on the number of canoes that came out to greet the *Golden Hind*, multiplying the number of men each type of canoe would carry with the total number of vessels. He explores the various types of canoes and paddles in use in the various Pacific islands mentioned as possibilities for Drake's island. Likewise, he examines such varied topics as personal ornamentation, clothing (or lack thereof), and weapons. The result is an interesting, informative, and useful study that explains and illustrates a valuable ancillary for certain aspects of history.

W. PATRICK STRAUSS  
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MICHAEL HUNTER. *John Aubrey and the Realm of Learning*. New York: Science History Publications. 1975. Pp. 256. \$28.00.

John Aubrey lived in a realm of learning deeply influenced by Bacon and Descartes, the realm of the Royal Society in which he was an active fellow from 1663 until his death in 1697, the realm of Harvey, Hobbes, Hooke, Petty, and Wren, who were among his close friends, and of Boyle, Halley, Lister, Locke, Newton, and Willis, who were among his acquaintances.

Aubrey rejected uncritical acceptance of old authorities and texts: "*Things* were not then studied. My Lord Bacon first led that dance." As one who separated faith and reason, he stood among the most advanced thinkers of his age: he thought stratigraphy could prove that "the world is much older, than is commonly supposed." He regretted that he published little of his work but prided himself on preserving information for posterity and on serving as a coordinator in his intellectual circles: "I perform the function of a whetstone, which can make the iron sharp though itself unable to cut."

Aubrey also respected past knowledge; he was "disdainful of traditional learning yet uncertain in rejecting it." While in his *Natural History of Wiltshire* and elsewhere he collected information relevant to his "homespun theory of environmental determinism," he could not always separate the effects of this determinism from "determinism by the stars."

In some ways Aubrey went beyond collecting and preserving and indicated new directions. The preface to *Wiltshire Antiquities*—unusual as almost wholly social history—reports the manners and customs of past ages and stresses visual and hearsay field evidence, such as revels, old wives' tales, and architectural detail. Also in *Monumenta Britannica*, "the first English book that can be called

'archaeological' in the modern sense," Aubrey used non-literary remains and thereby distinguished archaeology from other historical disciplines.

Most readers will agree with Michael Hunter's conclusion that "not least in the brave empiricism" with which Aubrey went about his work "he illustrates perhaps better than anyone the contradictions, aspirations and failings of his 'searching age'." The book, based chiefly on voluminous manuscript sources, gives us a fascinating and valuable insight into the half century when modern science was emerging.

WALTER L. WOODFILL  
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Davis

WILLIAM L. SACHSE. *Lord Somers: A Political Portrait*. Madison: University of Wisconsin Press. 1975. Pp. viii, 336. \$25.00.

John Lord Somers, Lord Chancellor under William III and leader of the Whig Junto, has long been without a scholarly biography. It may be impossible to write a complete account of Somers's life, for most of his private papers were destroyed a generation after his death. The surviving material, which is almost entirely impersonal, is best suited to an extended essay on his public career, emphasizing themes rather than chronology and touching on his private life only when it explains his politics. William L. Sachse subtitles his study "a Political Portrait" and says that his chief aim is "to provide an account of Somers's activities as a politician and jurist." Unfortunately, Sachse does not abide by the limitations he has set and attempts a full-scale biography. Little is known of Somers's early years, so Sachse fills the first two chapters with miscellaneous material not immediately relevant to a political portrait. More significantly, Sachse's chronological approach tempts him to draw unwarranted conclusions from insufficient evidence. In trying to determine Somers's politics between 1689 and 1693, Sachse catalogues his service on parliamentary committees, even when the committees had as many as sixty members and there is little indication that Somers was active in their deliberations. He served on the thirty-nine-member committee which drafted the Bill of Rights, and in a conference with the Lords he defended the Commons' word "abdicated" in reference to James II's flight. Surely it is too much to conclude from such evidence that Somers was "instrumental" in the framing of the measure.

Sachse's study becomes more persuasive when it enters the middle 1690s; as leader of the Junto, Somers was a man whose positions and utterances were recorded by his contemporaries. Sachse con-

vincingly describes Somers's political abilities: he was an effective parliamentarian through his knowledge of the law, his mastery of parliamentary procedure, and his willingness to spend long hours on legislation. Moderate, practical, and non-doctrinaire, he was in Sachse's well-chosen word "serviceable" to Parliament and the monarch. Only in his cry of "no peace without Spain" during the later stages of the War of the Spanish Succession did he cling to an unrealistic position. Even with its flaws, *Lord Somers* enlarges our knowledge of late-seventeenth and early-eighteenth-century politics.

DOUGLAS G. GREENE  
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SHEILA BIDDLE. *Bolingbroke and Harley*. New York: Alfred A. Knopf. 1974. Pp. 307, xv. \$10.00.

Like a pilot for a series on Masterpiece Theater, Sheila Biddle's book spins the fascinating tale of Bolingbroke versus Harley. As the two statesmen dart back and forth, cloaked in deep intrigue, the voice of Swift is heard and the viewer is given occasional glimpses of the pain-ridden, tenacious, dying Queen Anne. Biddle has written a fascinating account of these years, one from which any student of early eighteenth-century England will learn much. One wonders, however, whether or not it was really so damned exciting.

We are told that the two protagonists were destined to be violent rivals. Their differing personalities seem to have played some role in the inevitability of the conflict. Harley, we learn, was unassuming, self-effacing, and without pretensions. He was modest to excess. He suffered from chronic procrastination, and his passivity was tinged by a deep sense of resignation. On the other hand, he was courageous, learned, and deeply attached to his family. Bolingbroke was the perfect foil for the ordinary Harley. He was addicted to women, hypocritically religious, overly ambitious, elusive, and slippery. He was a flatterer, yet inventive and shrewd. Elegant and charming, he was also a consummate actor who saw all life as a play with himself constantly at center stage.

This, then, is the tension played out in *Bolingbroke and Harley*. It is the ever-recurring political drama between, on the one hand, the dull, commonplace everyman, whose role fits, as Biddle notes, the ordinary Englishman's notion of himself, and, on the other, the brilliant and flamboyant eccentric who dazzles but ultimately frightens people, especially conventional and pious queens. Another recurring political drama works its way through the pages of this book and is personified in its two leading characters. Biddle stages Harley as

the paradigm moderate and Bolingbroke as the zealot, the ideologue. Bolingbroke sees party as the key to politics, and all else is trivial. He seeks a government of pure party loyalists. Harley is the prudential moderate. He views loyalty to the queen, not allegiance to the Tory party, as the principal qualification for office.

Harley and Bolingbroke are symbols of the timeless clash of opposite personalities and opposing principles. Their cooperation for those few years in the Godolphin-Marlborough ministry was obviously forced. They were doomed to rivalry. In the later "division between these two great men," as Swift put it, all politics is epitomized. It makes for fascinating reading and potentially great theater, but one still wonders—was it really that simple and that exciting?

ISAAC KRAMNICK  
Cornell University

R. A. C. PARKER. *Coke of Norfolk: A Financial and Agricultural Study, 1707-1842*. New York: Oxford University Press. 1975. Pp. viii, 222. \$19.50.

This important study of a great English agricultural estate had its origins almost two decades ago in R. A. C. Parker's Oxford doctoral dissertation, together with an article on Coke of Norfolk in the *Economic History Review* (Dec. 1955). The finished product has been well worth the wait.

Although modern English historians under the leadership of Sir John Habakkuk (Parker's dissertation supervisor) have concerned themselves more with the study of estate archives than have their Continental counterparts, they have not produced book-length studies of single estates—apart from studies of the nonagricultural activities of a few great estates. Agriculture was, after all, the largest of England's industries in the eighteenth and in the first half of the nineteenth centuries, and it provided the fortunes of most English landowners. In spite of those croakers who persist in making dismal pronouncements about the futility of local studies, a thorough analysis of the economic and managerial history of an estate like that of the Cokes of Holkham is bound to be valuable. As Parker himself puts it, a "study of a single estate cannot establish new historical generalizations. It may, however, weaken or destroy old ones" (p. 199).

This Parker has certainly done. Where his article of two decades ago undermined what he calls "the heroic interpretation of an English eighteenth-century agricultural revolution" (p. 199), his book thoroughly demolishes it. Beginning his study with the early eighteenth century, he demonstrates conclusively by means of a detailed account of estate finances and management that



"much of the so-called agricultural revolution" (p. 60) took place on the Holkham estate not during the regime of Thomas Coke (1776–1842), where the heroic interpretation places it, but during the regime of his predecessor, Lord Leicester (1718–59). Coke's stature is thereby diminished. He remains a leading agricultural improver, gifted as a manager and advertiser rather than as a farming innovator.

This excellent book prompts one complaint. It is curiously reticent about the recent work in English estate history, notably Michael Thompson's *English Landed Society in the Nineteenth Century*. Parker might usefully have set forth his particular findings in the context of some of Thompson's generalizations.

DAVID SPRING  
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PAUL S. FRITZ. *The English Ministers and Jacobitism between the Rebellions of 1715 and 1745*. Toronto: University of Toronto Press. 1975. Pp. viii, 180. \$15.00.

In this concise and clear treatment of early eighteenth-century (mostly 1715–23) Jacobitism, Paul S. Fritz adheres to a position dichotomous to the Whig view that the Stuart threat was not to be taken seriously. Building upon the important contributions of others, he details the nature of that threat and shows that not only was the ministerial fear of Jacobitism genuine, but those same politicians effectively utilized the threat of conspiracy to maintain their own hegemony. Jacobitism, we now learn with as much detail as ever before, was a central element in the shaping of English policy. Beginning with the rebellion of 1715, Stanhope, Sunderland, Townshend, and especially Walpole acted with determination to prevent England from being unprepared. To rest but a moment was viewed as an open invitation to the Pretender. Walpole's pacifism can be explained in part by his passion to keep Jacobites—their opposites not being what Horace Walpole playfully called *George-abites*—without allies abroad. It seems that both fear and political opportunism are important in understanding this period; Fritz gives equal weight to both, though the evidence points to the latter as the more substantial motive.

Fritz shows how the ministers, faced with a plot, were prepared to accept the risks and "magnify out of all proportion an ill-grounded conspiracy for the purpose of securing certain political ends." The intrigue, the foreign situation, and the government's vigilance and reaction are all placed in perspective, but some important questions must be raised. Does the existence of plots after 1715 imply that they were just as dangerous as those before?

Was genuine ministerial fear fully justified? We are treated to a smorgasbord of Jacobite activity and government reaction. Some of that activity was serious (e.g., 1719), but a strong case is not made to illustrate that the fear itself was always warranted on the basis of the international situation (e.g., 1717). The plots reveal, however, that the Jacobites were used more than they were users.

Fritz does show, however, that there was significant Jacobite movement in England and Europe. Most of it was unsuccessful, but some was potentially dangerous to the stability of England and the Hanoverian monarchy. Fritz is at his best in detailing this interesting blend of motion, emotion, and, if you will, commotion.

ROBERT BLACKKEY  
California State College,  
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DUNCAN FORBES. *Hume's Philosophical Politics*. New York: Cambridge University Press. 1975. Pp. xiii, 338. \$27.50.

David Hume was a subtle and fecund thinker and writer, and the era in which he lived and wrote was doubly transitional. Philosophically, the changes were in the direction implied by that now-ambivalent, but once totally optimistic, appellation, "Enlightenment"; politically, mid-eighteenth century Britain was still moving toward the eventual resolution, through liberal constitutional monarchy, of the revolutionary constitutional struggles of the previous century. In calling his study *Hume's Philosophical Politics*, Duncan Forbes intends to convey the idea that he is setting Hume's interpretations of the politics of his time in the framework of the developing ideas and historical perceptions of the period. He is not attempting an analysis of Hume's philosophy of politics but is examining the way in which Hume exercised philosophical independence while maintaining his commitment to an elaboration of the British constitutional tradition, applicable to the changing politics of the early Hanoverian period.

Drawing upon the major philosophical treatises, but even more on the *Essays*, the *Letters*, and the *History*, Forbes carefully examines what Hume meant by the "experimental method of reasoning . . . [about] moral subjects," how he applied his basic philosophical concepts to the analysis of political institutions, and the way in which his *History* demonstrated the complexity and changeability of the sources and uses of political power. In doing so, Forbes corrects a number of misconceptions about Hume that have become part of conventional scholarly wisdom, including an overemphasis on his break with many aspects of earlier political thought, the uniformitarian and unhistor-



ical nature of his "science of man," and his Tory revisionism in later versions of the *History*. Not only do Hume's ideas about politics emerge as internally consistent with his broader philosophical doctrines, but his practical applications—the naturalness of obligation, the primacy of institutions, the appeal to moderation, and the necessity to address the historical circumstances of one's own time—are also consistent with his established philosophical positions as an epistemological skeptic and incipient utilitarian.

Although the book contains object lessons for the intellectual historian and is a useful source for the specialist in eighteenth-century British political history, its scholarly overkill is likely to disturb most readers. The background ideas of writers other than Hume are presented so elaborately, the digressions in both the footnotes and the text are so complicated, and the tendency to overquote and to paraphrase in almost verbatim terms is so pronounced that one tends to be diverted from the main line of argument. Thus, while many important interpretive points are brought out in the course of this near-variorum edition of Hume's political commentary, Forbes' interpretations are so embedded in excessive learned discourse that they fail to make the impact on the clarification of Hume's politics that Kemp Smith's cogent argument about the developmental relation between Hume's theory of moral sentiments and his conception of ideas made on the understanding of Hume's ethics.

WILLIAM C. HAVARD  
Virginia Polytechnic Institute  
and State University

BERNARD SEMMEL. *The Methodist Revolution*. New York: Basic Books. 1973. Pp. viii, 273. \$10.95.

Once again Bernard Semmel has put those of us interested in the history of modern England in his debt. In this difficult and challenging book he raises a number of stimulating questions that open new areas of inquiry, and he creates a broad context for the "Halevy thesis" that gives new life and significance to a problem that had become a trifle shopworn. The central thesis of *The Methodist Revolution* is that John Wesley developed a new theological synthesis that underpinned a widespread spiritual revolution of such social significance that it deserves to be seen as the English counterpart of the democratic revolutions then sweeping Europe and America. Peculiar to the Methodist revolution was an emphasis on order as well as on liberty, equality, and fraternity, and an abiding respect for law as well as a glorification of individual freedom. Methodism, Semmel argues, reached hundreds of thousands among the working classes and pre-

pared them to make a peaceful transition from traditional to modern society. Because he thinks that Methodism had the same relationship to nineteenth-century liberalism that Weber and Tawney ascribed to Calvinism and capitalism, Semmel bases his work on the premise that the theology of Methodism deserves the close study that Calvinist doctrine has received. Semmel's attempt to follow the footsteps of Weber and Tawney has led him both to success and to partial failure.

The discussion of Wesley's theology is the freshest, best-documented, and most convincing part of this book. Semmel demonstrates in detail how sharply Wesley's Arminian view of universal salvation differed from the Calvinist emphasis on a predestined elite, and he pleads persuasively that Methodist theology, in sociological terms, should be classified as modern in contrast to Calvinism, which should be considered traditional. At his best in showing how Wesley's personal background reinforced the thrust of his theological distaste for antinomianism, Semmel is, moreover, able to document solidly the fact that Wesley saw no significant gap between theology and behavior. For Wesley himself, the antinomian implications of predestination were pregnant with disorder and rebellion, while he identified free will and conditional salvation with sexual morality, commercial honesty, and loyalty to the king. For furthering our understanding of Wesley himself, the techniques of intellectual history that Weber and Tawney helped to develop seem appropriate and fruitful. For dealing with Methodists *en masse*, this style of history is less helpful.

Were the practical implications of the Wesleyan theological synthesis clear to the great majority of Methodists? Were the majority of Methodists even aware of Wesley's theological position? These are important questions within the frame of Semmel's thesis, but there are no satisfactory answers in this book. For Semmel, it is critically important to see the Methodist revolution as a widespread phenomenon involving many people. Its impact carried beyond the bounds of the denomination, he tells us, and he suggests that it was one of the more important forces that shaped nineteenth-century English society. But he sees the Methodist revolution in theological terms, and he cannot demonstrate that Wesley's theology was understood or even apprehended beyond a small circle of well-educated leaders. Semmel is, indeed, driven by the evidence to point out that at the time of Wesley's death, the position of many Methodist congregations and preachers seemed more Calvinist than Wesleyan. Evidence for a broadly based anti-Calvinist march toward modernity is not forthcoming. Semmel does not come to grips with this problem, nor does he consider the possibility that the experi-

ence of most men and women who were caught up in the evangelical revival may have had little relation to systematic theology. The best part of this book is the treatment of Wesley's theology. The next best part is the set of stimulating questions that it provokes about the nature of Methodism, the concept of modernization, and the historical role of ideas and the limitations of intellectual history.

R. J. HELMSTADTER  
University of Toronto

correspondence and legal files. He has apparently used all the available source materials, and his narrative is well-documented with extensive footnotes and appendices. Buchanan writes with clarity and even his explanations of the legal machinations involved in assembling the papers move briskly. Carefully researched and organized, the book is a lively and intelligent study of one of the most important literary discoveries of this century.

BETTY M. DUFFY  
Louisiana State University

DAVID BUCHANAN. *The Treasure of Auchinleck: The Story of the Boswell Papers*. New York: McGraw-Hill Book Company. 1974. Pp. xxii, 371. \$14.95.

James Boswell believed in documentation. As early as 1758 he started to record his experiences, and in 1762 he began the first of a series of journals which he maintained until his death in 1795. They eventually covered more than eight thousand manuscript pages, and Boswell used them as a repository from which he drew most of his writings, including the *Life of Johnson*. Moreover, Boswell carried on a voluminous correspondence, keeping the letters he received and making copies of many of his replies. He also preserved his original manuscripts and additional notes, memoranda, and anecdotes (especially those concerning Johnson), as well as documents relating to his family, his estate at Auchinleck, and his legal practice. Because his correspondents included many prominent men, the papers not only provide a painfully candid self-portrait of Boswell himself, but also open a window on eighteenth-century Europe.

David Buchanan traces the history of the papers from their purported burning by Boswell's family to their preservation at Yale. The hero of the book is Colonel Ralph Isham, a determined collector and literary detective, whose pursuit of the documents lasted twenty-one years and cost him his marriage, his home, and his fortune. Isham acquired the papers owned by Boswell's descendants at Malahide Castle in Ireland and began an expensive plan of private publication only to discover he did not possess the entire collection. More documents kept turning up at Malahide—in a granary loft, an old deed box, and a croquet box—and then a major new find occurred at Fettercairn House in Scotland. As a result, Isham became embroiled in a costly and lengthy legal battle to obtain the additional papers. Ultimately successful, he was able to complete his collection and to achieve his goal—to make the papers available to scholars at a university equipped to publish and edit them.

Buchanan, a lawyer whose father represented Isham in Scotland, had access to Isham's own

J. E. COOKSON. *Lord Liverpool's Administration: The Crucial Years, 1815-1822*. Hamden, Conn.: Archon Books. 1975. Pp. xiii, 422. \$15.00.

This study fills in most of the Liverpool years preceding W. R. Brock's older work on Liberal Toryism, 1820-27. The era is remembered chiefly for the Congress of Vienna and the Concert of Europe, Spa Fields and Peterloo, Arthur Thistlewood and Queen Caroline. Cookson virtually ignores the first two, and uses the others to illuminate the vagaries of contemporary politics.

It was a period when the monarch, cabinet, and opposition were all uncertain regarding their roles, and even the identity of their supporters. Adding to the confusion was the growing influence of public opinion. The Whigs in bidding for power stressed the responsibility of Parliament to the public mind, which they sought to control, while the Tories stubbornly attempted to maintain a fair share of initiative for the executive. Cookson concludes that it was not until the Queen Caroline incident that the latter came fully to terms with public opinion.

Lord Liverpool emerges as a practical politician who was well-equipped to cope with the situation. Despite his Toryism, the prime minister firmly believed that kings should be seen, not heard.

The economic aspects of the period, the heated quarrels over postwar budgets, and the persistent demands for retrenchment are treated in considerable detail. Social issues, save for the reform of the penal code, are not emphasized.

Cookson's primary sources are many and illuminate, for example, such an elusive political faction as the Grenvilles. The writer also refers to numerous secondary sources, though in this area his acknowledgements of previous contributions seem somewhat perfunctory.

It is a relief to read a study of this era that does not downgrade the Liverpool Government, and cast the Whigs and Radicals in the role of far-seeing pundits. By and large, it is objective, balanced, nonpartisan and convincing—a work for specialists, studded with facts and figures, as well

as some insights into early nineteenth-century British political and economic life.

WILBUR DEVEREUX JONES  
*University of Georgia*

SUZANNE MIERS. *Britain and the Ending of Slave Trade*. New York: Africana Publishing Company. 1975. Pp. xv, 405. \$25.00.

This is a work which appears from Suzanne Miers's prefatory remarks, to have begun as the study of a Neglected Diplomatic Event, the 1889-90 Brussels Anti-Slavery Conference, and then shifted its focus to a Broad Social Issue, the entire nineteenth-century European attack upon the African slave trade. The two objectives are not entirely contradictory, and in general this very soundly researched book succeeds in its goals.

The Brussels Conference itself occupies only about one-third of the volume and is disposed of comprehensively, clearly, and unpretentiously. Its major achievement, Miers points out, lay not in the professed program of abolishing the slave trade and limiting the import of liquor and arms to Africa; such philanthropic aims fell victim to the political and economic rivalries among the participating powers, many of whom were concurrently engaged in establishing claims to African colonies. The very convocation of this (and the preceding Berlin West Africa) conference did, however, succeed in establishing a system for settling international affairs that would be more fully realized in the later League of Nations and United Nations. Thus the prime forum of today's Third World traces its paternity to the Diplomacy of Imperialism!

In her lengthier examination of the (mainly British) anti-slavery campaigns before 1889, Miers considers not only the traditional issue of humanitarian versus material motives on the part of Europeans but also the relevance of slavery and the slave trade to contemporary African societies. The material here is rich and again well-presented, and the author's arguments generally sound. In some ways the caution that Miers uses in interpretation is a welcome relief from the strident liberal-radical polemics so common in recent historiography on European-African relations, but some readers will be disappointed not to find a stronger effort at a general assessment of British aims. Miers does show how humanitarian, commercial, and strategic goals could coincide in the period before 1880 in protecting weak colonies in West Africa and the Red Sea. In any case, not much was at risk in trying to combine such efforts even when they were not in perfect harmony. But once other European powers entered seriously into the African scene, Britain needed to make sure that anti-

slavery action served to justify her own more aggressive posture and did not give advantages to less principled rivals. Thus the Brussels Conference, in all its confusion of cynicism and idealism, really did constitute the climax of a major trend in expansionist policy.

Miers's lengthy section on slavery within Africa (drawn to a considerable extent from a separate volume of ethnohistorical studies that she and Igor Kopytoff are editing) effectively demonstrates what the abolitionists never understood very well: that slavery and the slave trade could be assimilated to existing African social structures and values, and often represented the very form (although still quite different from New World plantation models) by which Africans adapted to the labor demands of European-inspired "legitimate" economic opportunity. In fact the colonial governments put into place after the Brussels Conference proceeded rather cautiously with the attack on local slave systems because they were forced to recognize their importance to Africans.

Miers makes little attempt to investigate why anti-slavery sentiment remained an independent force in formulating European policy toward Africa. Only in discussing (from the primary research of others) the abolitionist campaign of the French Cardinal Lavigerie does she suggest that the very humanitarian appeal of such an undertaking may have served certain domestic social purposes in Europe. Greater attention to the "social imperialism" of the anti-slavery movement might have provided a more unified basis upon which to comprehend the mentality of the British and other statesmen involved in making hard decisions about Africa.

RALPH A. AUSTEN  
*University of Chicago*

ROBERT A. HUTTENBACK. *Racism and Empire: White Settlers and Colored Immigrants in the British Self-Governing Colonies, 1830-1910*. Ithaca: Cornell University Press. 1976. Pp. 359. \$17.50.

The disparity between metropole liberalism and harsh colonial-frontier attitudes frequently exposed in "native" policy studies is now extended to relations between white colonists in Australia, New Zealand, Canada, and South Africa, and indentured and free Chinese, Indian, Pacific Island, and Japanese immigrants. London, espousing an imperial philosophy that in Joseph Chamberlain's words "makes no distinction in favour of, or against any race or color," found it impossible to enforce equal rights or humane conditions in the face of settlers whose isolation bred a perception of themselves as beleaguered outposts of Anglo-Saxonism.

Three themes predominate: white settler reaction with its vicious manifestations; legal and constitutional relations between London-based Colonial and India Office officials with colonial governments; and Foreign Office contacts with sovereign Chinese and Japanese diplomats dismayed by legislation that excluded their nationals and infringed civil liberties of those already domiciled in the colonies. Australia pioneered the limitation of immigrants on the basis of ship tonnage, but all colonies later shifted to the "Natal formula" of literacy in a European language.

The author's claim that cultural and genetic inferiority were founded in virulent racism rather than the conventionally ascribed fear of cheap labor is weakened by the evidence of frequent capital-labor splits on policy, indicating that political and biological philosophies of exclusion reflect underlying divergent economic interests. Primary sources are drawn from London as well as pertinent Commonwealth archives. It would have been revealing if these records indicated migrants' motivations, especially since a "normal" five percent annual mortality appears to have prevailed among Asian migrants in Australia.

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H. J. DYOS and MICHAEL WOLFF, editors. *The Victorian City: Images and Realities*. In two volumes. Boston: Routledge and Kegan Paul. 1973. Pp. xxxii, 428; xii, 431-957. \$85.00 the set.

Every historian of nineteenth-century England owes a deep debt to the two editors of this handsome, lavishly illustrated book. H. J. Dyos has done more than anyone else to organize and inspire the study of urban history, in England and out, and he has contributed notably to it through his own research and writing. Michael Wolff was a founder and for long the principal editor of *Victorian Studies*. A conference they organized at Bloomington, Indiana, in 1967 led to this book, though only four of the papers are substantially carried over.

At Bloomington, the editors suggest, the disciplines were not transcended, nor are they here. Most readers are certain to be more regretful about what had to be left out than are the editors in their cheerful and refreshingly unapologetic introduction, the more so as some of the essays are only distantly relevant: for example, G. H. Martin's and David Francis' historical sketch of photography; the essay by Wolff and Celina Fox on illustrated papers; Asa Briggs' introduction to the history of statistical inquiry; or John Kent's study of the failure of religion to appeal to the urban

working class. Useful though they are, these chapters tell us less about the city than might a couple of essays on any of the half-dozen major topics almost entirely ignored, among them government, schools and urban universities, and middle-class urban culture. Liverpool is scarcely here at all, nor are we shown what happened in cities, like Norwich, that declined during the century and had to find a new destiny. The bias of the book, then, is toward London and Manchester, toward the later rather than the earlier Victorian period, and, among the inhabitants of cities, toward the working classes and the problems they presented—a bit of twentieth-century distortion both more excusable and more rewarding than the preoccupation of some of the authors with sex. Eric Trudgill's chapter on prostitution is exemplary in its hard-headed good sense, but Steven Marcus, G. Robert Stange, George Levine, and Richard Schoenwald seem only trendy.

Schoenwald's extended metaphor interprets the sanitary movement of the second quarter of the nineteenth century as an exercise in national toilet-training—an idea that can make one think again about the centrality of social disciplines—but the argument as it is presented here will not stand up against an examination of what the sanitary reformers were and did and thought and said they were doing. Steven Marcus' pretentious essay on Engels' vision of Manchester suffers from similar faults. He draws an interpretation of the modern condition from Engels' interpretation of the shop-lined main streets of the city as a deliberate concealing of urban misery from the bourgeoisie as they whirled into the city from their suburban retreats. This foreshortening would not even survive a reading of the following chapter, Jack Simmons' superbly concise treatment of the railways, for as soon as the suburbanites began to travel into town by train, they saw only the worst—unless, of course, the *Manchester Guardian* was a Potemkin paper. An important and complex subject, the gradual separation of classes in the towns, is dealt with only in caricature.

A number of the literary essays in the book are similarly blinkered. Nicholas Taylor's chapter on the city as embodiment of the sublime is marvelous, but nothing like his awareness emerges in the attempts here to explain the resistance of Victorian poets and novelists to the city. Not nearly enough weight is given to the perpetual rural bias of the English or to the pull of Romantic sensibility. Instead we get Stange's fatuous suggestion "that the city becomes available as literary material only when a writer escapes the restrictions of Anglo-Saxon, Protestant gentility" (p. 481).

Some of the literary essays, though, are splendid—Philip Collins' loving tribute to Dickens as a



London man, P. F. Keating's account of the East End of London as a literary subject—and much else in the book is essential reading, more, alas, than can be noted here.

As a whole, the book suggests that we have far to go before the historical reality of the Victorian city can be encompassed. It may be, too, that on present showing, the old ways of approaching the subject are the best. My quarrels with this book, even about omissions, would be a polite differing with editorial priorities if it were not for one fault that genuinely compromises its authority: the rage in a few authors for novelty and innovation at all costs. Samuel Johnson, that great lover of cities, would not have been surprised.

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E. G. WEST. *Education and the Industrial Revolution*. New York: Barnes and Noble. 1975. Pp. 275. \$15.00.

E. G. West purports to show the interrelationship of education and the Industrial Revolution during the nineteenth century at a time when Britain's economy still was underdeveloped. In reality, the text is a historical treatise on views on education involving proponents of a state-supported system of education versus those who wanted a continuance of private education with some type of state aid. West says that before there was state help most children were being adequately educated with parents providing funds. With state financing, the share of national income spent on schooling did not increase proportionately. Scottish elementary education is discussed and the belief disallowed that "Scotland enjoyed substantially better investment and progress in education compared with England" (p. 72).

Available primary sources are used to show bias evidenced by misuse of the "school-age population base" in order to bring about state intervention. Classical economists' views for state-sponsored education are given as well as others, including statements of Charles Dickens and Tom Paine. Dickens was for national education. His literary interpretation of "abusive" private education is given, then he is dismissed with the statement, "Perhaps schools . . . generally will always be the victims of the literati" (p. 54). More good has been written about English private education than West allows. Tom Paine favored remission of taxes to the poor and allowing them use of money for their children's schooling. Later education came to be tied in with issues like the Poor Laws and the reduction of crime.

In quantitative terms the Industrial Revolution

brought educational growth, but "the relationship between [the two] . . . is unexpectedly complex . . . [because] the data does not readily lend itself to sophisticated treatment" (p. 245). In this work some effects of education on the economy are mentioned, one being that the more education one had, the greater personal income one commanded. Once the state supplemented private education, state supervision arrived, along with state board schools. Soon the latter dominated the scene and forced the closing of some private schools. Education then became truly national and "mass."

KATHLEEN E. DUNLOP  
East Carolina University

KEITH A. P. SANDIFORD. *Great Britain and the Schleswig-Holstein Question, 1848-64: A Study in Diplomacy, Politics, and Public Opinion*. Buffalo: University of Toronto Press. 1975. Pp. x, 204. \$15.00.

Keith A. P. Sandiford is one of those historians who would have written a better book if they had thought their subject more complex. Analysis of British diplomacy on the Schleswig-Holstein question has been less neglected than he claims, but only very superficial attention has hitherto been given to British public opinion regarding that issue, or to defining how this affected government pronouncements or policy. I therefore regret that Sandiford devotes his main effort to constructing a clear, solidly based diplomatic narrative. His efficient reworking does not substantially alter the conclusions already put forward. Also, although he emphasizes that his standpoint is not the usual European one but purely British, he fails to place the politicians' disagreements over Schleswig-Holstein in 1863-64 in the context of the perennially evolving British debate on intervention in Europe in which most of the senior politicians had taken their positions twenty years or so before. In contrast, his treatment of British politics and opinion is scrappy and narrowly based, and fails to practice the heuristic principles preached at the end of his opening chapter. For example, essential though it undoubtedly is to distinguish public opinion as revealed by reports of petitions and public meetings, letters to the press, and other sources from the "line" taken by the organs of the press (which in the case of the London dailies was even more closely influenced by a few politicians than is suggested here), nevertheless Sandiford deals unequivocally with public opinion as distinct from press or parliamentary opinion only once (pp. 97-8). Of the twenty-five journals appearing in the bibliography, he cites only six in the footnotes. And he never explores the real determinants of the various strands of thought and feeling at work, just as he never explains the party politics

behind the vote of censure moved in July 1864 and never assesses the political weight of the different speakers (or indeed of newspapers and periodicals). In the ten years since he finished his thesis, the question of how far British government in the mid-nineteenth century was open to outside pressure has become very much a leading one. Thus I am all the sorrier that his finished book leans so heavily toward diplomacy and has so little to add to our knowledge or understanding of politics and public opinion.

OLIVE ANDERSON.  
*University of London,*  
*Westfield College*

JOHN GRIGG. *The Young Lloyd George*. Berkeley and Los Angeles: University of California Press. 1973. Pp. 320. \$12.00.

More has been written about Lloyd George than about any other recent British politician except Winston Churchill. Few of the many biographies of the Welsh statesman are even moderately satisfactory, and most are shoddy, being mere hagiography or the products of vendettas. None has explained properly the reasons for Lloyd George's rapid rise from obscurity to almost unparalleled dominance during World War I. His early career particularly remains enigmatic.

John Grigg remedies many of the defects of previous biographies, though some important family papers were closed to him. These family papers may not change interpretations significantly, since much of his family correspondence, such as that between Lloyd George and his first wife, tends to be trivial and unrevealing.

Grigg's achievement lies not in new documentation, but in judicious re-examination of previously published material. This is especially noticeable when he deals with Lloyd George in the context of Wales in the 1870s and 1880s. Welsh historians have stressed the importance in Lloyd George's career of his activities in some rather small nationalist movements of those years. Grigg shows that his horizons went far beyond Wales even in his earliest days in politics. Grigg also demonstrates that Lloyd George's origins were not as humble as most previous writers have maintained, and that his family was prosperous and prominent in relation to others in his home village.

The author uses one source which few other historians have consulted, the manuscript memoir, in Welsh, of Lloyd George by D. R. Daniel. This interesting document should be looked at by every historian of the period, since it is full of small but revealing details about Lloyd George's habits, his likes and dislikes.

Those who expect revelations about high politics will be disappointed in this book: it ends with Lloyd George on the verge of power, and in any case, Grigg's emphasis is on Lloyd George's personality. It is a good old-fashioned biography of the best sort—readable, thoughtful, and fair. It makes an auspicious beginning to what will presumably be a trilogy by John Grigg.

MICHAEL KINNEAR  
*University of Manitoba*

WILLIAM J. FISHMAN. *Jewish Radicals: From Czarist Stettin to London Ghetto*. New York: Pantheon Books. 1975. Pp. xvi, 336. \$12.95.

The Jewish immigrants who came to England from Russia and Poland during the last thirty years before the First World War congregated overwhelmingly in the East End of London. Although they numbered some quarter million at most, their highly visible contributions to urban overcrowding and to sweated labor in the tailoring and bootmaking trades made them the target of genuine social reformers and simple xenophobes alike, while friction with their English neighbors and fellow workers in the East End made their lives more difficult. At the same time, as William J. Fishman very properly points out, much of the blame for the state and activities of the immigrants lay outside their control. At bottom, the immigrants were products of a repressive and economically backward nation that not only failed to prepare them for integration into a highly industrialized, urban society, but forced them to depend upon their own culture and heritage so completely that at first they could not adapt themselves to English conditions.

The importance of the libertarian movement among the immigrants, in Fishman's view, lies partly in the attempts it made to ameliorate their miserable conditions, but much more in its attempt to inculcate in the immigrants its own conception of a just, equitable society based upon free choice and free association. Its strong influence in the Jewish labor movement is to its credit, but as the author points out, its failure to make headway in the greater task of proselytizing, and its destruction by World War I, ought not to condemn it to oblivion.

The work would have benefited from more detailed discussion of anarchist theory in general and its relation to Jewish theorists (especially the important but neglected Rudolf Rocker), and from comparative discussion of the development of this and contemporary anarchist movements among New York's Jews, and in the artisan and peasant cultures of Italy and Spain. Nevertheless, it is a useful addition to our knowledge of anarchism in



action, drawing together much published material and interviews with survivors of the movement.

BERNARD GAINER  
University of Kansas

PAUL THOMPSON. *The Edwardians: The Remaking of British Society*. Bloomington: Indiana University Press. 1975. Pp. xi, 382. \$15.00.

This well-researched monograph is the first to provide detailed information about the basic dimensions of social change in early twentieth-century Britain. Its appearance is most welcome because, as Paul Thompson himself asserts, "from the point of view of social history, the early twentieth century still lies in an academic limbo." Dramatic accounts of the turbulence during the Edwardian era are no longer sufficient, according to the author, because they say little about the impact of that turbulence on the daily lives of specific Englishmen.

He feels that he has corrected this flaw by writing a book that is "both sociology and history." While admitting that "real knowledge of British society in this period rests on a very slender basis," he still pushes forward by probing into the experiences and contributions of some five hundred ordinary Edwardians through the effective use of the "interview method" that he defends. He hopes to establish the most important dimensions of social change in the Edwardian era and, in the process, to show the extent to which the reasons for that social change were conscious efforts. Yet, he does not neglect to evaluate, in what I judge the most valuable part of the book, all of the chief "instruments of change," be they conscious or unconscious. Meanwhile, in a series of deeply informed and often detailed profiles built on their own words, he has examined a cross-section of twelve Edwardians of all classes so as to place in the evaluation of general social change "the contribution and experience of ordinary individuals."

Anticipating criticism for this minimal sampling, the author willingly acknowledges that the book represents an "interim interpretation and not a conclusion." Most readers will probably agree, however, that despite the selection of only twelve, the Edwardians chosen do effectively provide the "dimensions of inequality" in Edwardian Britain, and in doing so show how such dimensions were inherently behind the moves for social change in that world. Criticisms are likely nonetheless. Some may find it irritating that, while generally examining the "dimensions" in part 1, he makes specific references to one or more of the profiled Edwardians that the reader will not become acquainted with until part 2. And, in the final chapter, where the sociologist in him prevails over the historian,

his theorizing may not be well received by historians. In his words, "some readers may wish to skip" these paragraphs.

It must be emphasized, however, that in light of the contribution this book makes to a fuller understanding of Edwardian society, these flaws are minor. The flavor of Edwardian society comes through, and, taken as a whole, the book provides a more human picture of the Edwardian era than has yet been seen. This should be welcome news to historians and sociologists alike.

RAYMOND G. HEBERT  
Thomas More College

J. C. MASTERMAN. *On the Chariot Wheel: An Autobiography*. New York: Oxford University Press. 1975. Pp. x, 384. \$14.50.

Life was good to J. C. Masterman. After a First at Oxford, he was appointed with minimum fuss to a lectureship at Christ Church. Trapped in Germany in 1914, he spent the First World War in Ruhleben, a "not intolerable" prison camp. After the war, he returned to Christ Church where he taught modern history, played tennis and cricket on the international level, and assumed a variety of administrative duties. In the 1930s he published his first works: a detective story, a novel of manners, and a historical play, which was performed only once. During the Second World War, he served in British intelligence, part of whose operation he later described in *The Double-Cross System*. Returning to Oxford after the war, he became provost of Worcester and later vice-chancellor of the university. He also published another detective novel and a guidebook to Oxford, which was well received. In 1961, at the age of seventy, he accepted a lucrative post in industry, finally retiring six years later. "Always, as I now realize," he concludes, "I have been a privileged person and I rejoice in the fact."

Sir John writes with clarity, wit, and a disarming charm. He is unfailingly courteous in his evaluations of other men. His anecdotal description of Oxford is in the best tradition of nineteenth century memoirs. He does not hide his elitist views on education, stressing that a system of competitive examinations often leads to "mediocrity." The title of the book refers to a fly on an axle wheel who cries, "What a dust I have raised."

D. I. IEMAHEU  
Lake Forest College

ERIC ASHBY and MARY ANDERSON. *Portrait of Haldane at Work on Education*. Hamden, Conn.: Archon Books. 1974. Pp. xvi, 202. \$10.00.

Lord Haldane regarded his work on education as the most important aspect of his career; he wanted

to be remembered for it. Ironically, historians have remembered him more as a Liberal, as the friend and colleague of Asquith and Grey, and as a war minister. His educational reforms have been largely neglected. *Portrait of Haldane* now fills that gap in our knowledge. Haldane's impressive educational accomplishments more than justify this study. He helped to reform the University of London and to create the present university grants system and the present system of civic universities. He paved the way for Fisher's Education Act of 1918, and he contributed to securing public recognition of the importance of adult education. In short, Eric Ashby and Mary Anderson contend, he laid "the foundations of our whole system of education." Their work is brief and admirably well written, conveying a sense of Haldane's character and methods of work. And as a portrait of Haldane the book will be useful to historians not specifically interested in educational reform.

For all its virtues, *Portrait of Haldane* is a limited historical work. Much of the story is presented in a vacuum. The reader is often left with the impression that educational changes are brought about almost solely by the actions of well-meaning individuals. Larger social and political forces are too often ignored. The consequence is to make Haldane sometimes seem larger than life. This tendency is re-enforced by the authors' sympathetic view of their subject. No doubt their attitude contributes to the book's strong points, but it also leaves much room for a more critical examination of Haldane's elitist and imperialist attitudes and his view of education as a "tranquilizer" of the masses.

PETER WEILER  
Boston College

KENNETH J. CALDER. *Britain and the Origins of the New Europe, 1914-1918*. (International Studies.) New York: Cambridge University Press, for the Centre for International Studies, London School of Economics and Political Science. 1976. Pp. viii, 268. \$19.95.

This is a study of British policy towards the Polish, Czechoslovak, and Yugoslav movements for independence during World War I.

The most striking change in the map of Europe after World War I was the appearance of a band of new national states in the area from the Baltic to the Adriatic Sea, previously occupied by the multinational Habsburg empire which had disappeared altogether and the similarly heterogeneous Russian empire which had been much diminished. In general surveys of European history, it is usually explained that this "new Europe" was created at the Paris Peace Conference to implement the prin-

ciple of national self-determination, which President Woodrow Wilson had enunciated in his famous Fourteen Points address on January 8, 1918, and which all the Allied powers had adopted as the guiding principle of their peace programs.

In this book, which was originally a doctoral dissertation, Kenneth J. Calder limits himself to the study of British policy towards the Polish, Czechoslovak, and Yugoslav independence movements. It is his thesis that British policy toward these three nationalities was not based on the principle of national self-determination but on expediency. At the beginning of the war, the British government had no policy toward these nationalities; indeed, it was scarcely aware of them. As the war lengthened and became a desperate test of strength between the two groups of belligerents, however, the British government found it expedient to use the Polish, Czechoslovak, and Yugoslav exile committees in Paris and London in the prosecution of the war. By the end of the war, it found itself morally, if not legally, bound to support their independence. Thus war expediency rather than planning for peace guided the British government in its policy toward Austria-Hungary and its disaffected nationalities.

Students of World War I diplomacy will not find Calder's thesis strikingly new, for the eminently pragmatic nature of British wartime diplomacy has long been apparent. Undoubtedly, however, they will welcome Calder's book, because it is the first systematic study of the subject based on unpublished British government records, which have been opened to the scrutiny of historians only recently. The principal weakness of Calder's study is that it treats British policy in complete isolation. He makes no attempt to examine the interaction of British and Allied policies toward the three nationalities or the response of these nationalities toward those policies; the result is a singularly insular view of the problem. Another weakness of the book results from Calder's failure to consult published sources and previous literature on the subject, which distorts his perspective and limits his understanding of it. These weaknesses unfortunately bar an otherwise commendable effort from being the definitive study of the subject.

VICTOR S. MAMATEY  
University of Georgia

STERLING J. KERNEK. *Distractions of Peace during War: The Lloyd George Government's Reactions to Woodrow Wilson, December, 1916-November, 1918*. (Transactions of the American Philosophical Society. New series, volume 65, part 2.) Philadelphia: the Society. 1975. Pp. 117. \$6.00.

If ever a work of scholarship has tended to confirm the popular stereotype of "Perfidious Albion," it is

Sterling J. Kernek's study of the reactions of Lloyd George's wartime government to Woodrow Wilson's peace initiatives, and finally to his war aims. The First World War created dilemmas for British policy-makers in their dealings with the U.S. On the one hand, Britain required immense quantities of American capital and war materials, yet offended Americans through flying in the face of attitudes toward neutrality and freedom of the seas as old as the Republic itself. On the other hand, while the British Government never intended a negotiated settlement, President Wilson demanded a "Peace without Victory" for both sides in the struggle.

The central thesis of this book is that, in their dealings with the president, the British easily achieved their major goals; indeed, their achievement might serve as a classic example of preventive diplomacy and crisis management. Until the United States became involved in the war as a combatant, the British were able to parry Wilson's peace initiatives and exploit the nation's unwillingness to see Germany victorious. After the U.S. cast its lot with the Allies, they utilized Wilson's idealistic war aims—self-determination of peoples, no territorial aggrandizement, etc.—to neutralize pacifist criticism and peace moves from other quarters. In the end, British interests were preserved without substantive concessions being made to the Wilsonian program.

From a literary standpoint, this book is written in a plodding, uninspired style. As a research effort, however, it is a solid, meticulously prepared work that should stand up well for many years to come. The author has judiciously blended new materials, primarily Foreign Office and cabinet documents opened to scholars since 1967, with papers in well-known manuscript collections: David Lloyd George, A. J. Balfour, Lord Robert Cecil, Woodrow Wilson, Colonel House, Robert Lansing, and others. Kernek's lapses are minor. For example, to call Lloyd George (p. 8) "the Welsh politician" is like calling Campbell-Bannerman "the Scottish politician" or George V "the German monarch." Lloyd George, if he was anything, was a *British* politician of great astuteness—although he was not astute enough to prevent the Liberal debacle after the war. It would be unfair to fault the author for omitting what it was never his purpose to achieve, but the book would have profited from a discussion of the extent to which inter-governmental dealings were influenced by domestic pressures, especially from the Union of Democratic Control and the Labour party in England, and progressive intellectuals, notably those associated with *The New Republic*, in America.

ALBERT MARRIN  
Yeshiva College

SIDNEY and BEATRICE WEBB. *A Constitution for the Socialist Commonwealth of Great Britain*. With an introduction by SAMUEL H. BEER. Reprint. New York: Cambridge University Press in association with London School of Economics and Political Science. 1975. Pp. xlv, 364. \$16.95.

BEATRICE WEBB. *Our Partnership*. Edited by BARBARA DRAKE and MARGARET I. COLE. With an introduction by GEORGE FEAEVER. Reprint. New York: Cambridge University Press in association with London School of Economics and Political Science. 1975. Pp. liv, 544. \$21.00.

SIDNEY and BEATRICE WEBB. *Methods of Social Study*. With an introduction by T. H. MARSHALL. Reprint. New York: Cambridge University Press in association with London School of Economics and Political Science. 1975. Pp. xlv, 263. \$17.95.

The student of British history who visits Westminster Abbey and sees the memorial place of the Webbs will probably agree that this honor was richly deserved. The celebrated pair's devotion, *pro bono publico*, as publicists, researchers, and reformers, extended over half a century and surely resulted in the "considerable work" Beatrice had hoped for in 1892. On the other hand, that same visitor might recall Shaw's campaign to make the Abbey the Webbs' final resting place and ruminate that it was a last, a posthumous feat of permeation. Such ambivalent thoughts come to mind in contemplating the three books under review here, all reissues and the first installment of the publishers' plan "to make available once more those books by the Webbs that are still of value."

The field of Webb and Fabian studies, divided between disciples and cultists as against iconoclasts and debunkers, remains in a decidedly unsatisfactory state. The first urge that Webb and Fabian "influence" was paramount, the latter deny them any importance, arguing that all legislation and social changes claimed by them, or for them by their admirers, would have been effected had the Webbs never been born and the Fabian Society never founded. Both views depend largely on speculation. There is, consequently, very little work on the Webbs—most of it is confined to a few relatively short essays—that measures up to the canons of scholarship, detached, searching, eschewing the merely personal, and helpful to the student who seeks to establish the particular significance (or lack of it) of the Webbs in each of the many and varied fields in which they were active.

The *Constitution* is the most important of these three books for general history, although what T. H. Marshall says in his introduction to *Methods of Social Study* is essentially true of all three: They will be read more for what they reveal about the Webbs than as an exposition of their ostensible subject matter. The *Constitution* can be a source of

bafflement to the student of the Webbs in that, here, they offer a thoroughly democratic solution to all questions; the book appears, therefore, to be inconsonant with so much else that the Webbs wrote and did—particularly their entering the Soviet tabernacle in old age.

One clue to the authoritarianism of the Webbs and their embarrassing embrace of the Soviets' "New Civilization" is provided in Samuel H. Beer's perceptive introduction on "The Ideology of the Webbs," that evolutionary sociology and evolutionary rationalism which derived from Spencer and Comte. A further clue will be found in the dualism expressed in a letter to Bertrand Russell in the early years of this century in which Beatrice insisted upon "an absolute distinction between the realm of proof (Knowledge of Processes) and the realm of aspiration or Faith (the choice of Purposes)." All her life she hungered for a "Faith." And when the old faiths failed in war and depression, she turned to the gospel according to Stalin, she and Sidney paradoxically and perplexingly declaring that Soviet Russia conformed to their democratic, antibureaucratic, anticentralist prescriptions in the *Constitution*. H. G. Wells' reference to the "Chestertonian facet" of her "faceted mind" was an acute observation.

The *Methods of Social Study* is a relatively slight work, of interest because of its revelation of the research methods used by the Webbs, a kind of preface to the work that is most likely to make their name live, the massive history of local government. Compared to a standard manual, say Barzun and Graff's *Modern Researcher*, the *Methods* is as remarkable for what it includes as for what it ignores. When something is illustrated by "personal experience" or by the vivid pen portraits drawn from Beatrice Webb's diaries, it leaps to the eye; the plodding exposition of the book as a whole, however, suggests that the "firm of Webb" represented a marvelous combination of talents, but that it produced those hundreds of books, articles, and pamphlets at the price of style.

*Our Partnership*, by contrast is a work of great literary distinction, though less so than *My Apprenticeship* because it is necessarily so diffuse. This reissue is, except for George Feaver's sketch of the pair's lives and character, exactly the same as the first edition. This means, for example, that the biographical index supplied by its first editors remains unamplified and uncorrected; not even the date of death for those who died since 1948 is supplied. More important, none of the excisions made in the diary selections has been restored. It appears that there are no plans to publish the diaries complete (though there are for the Webb correspondence, which will be a great boon), and so scholars of the period will continue to make the

pilgrimage to the library at London School of Economics to read the Webb diaries in the precincts of the school they founded.

It is only in the complete diaries that one can follow the spiritual quest that ultimately carried them into the church of the "New Civilization": From 1896, "*We must create a new 'Orthodoxy'*" to 1935, "Yesterday Sidney made the first draft of the final paragraph of our book . . . it is 'The End': A thrilling moment, when he read it to me! Our last will and testament. . . . We have done the work we intended to do, in blessed partnership. What more can mortals want?" The mortal historian can only wish that this passage had set the seal not on *Soviet Communism: A New Civilization*, but on two equally portly volumes on the history of local government since 1835. As it is, apart from their work on the Poor Law in "the last hundred years," we only have five lectures Sidney delivered at London School of Economics in 1899; they were prepared before their research on local government had really begun, and we have them only in barest summaries; nevertheless, they will be worth having, and we hope they will be reprinted in the present highly commendable series.

FREDERICK M. SCHWEITZER  
Manhattan College

HENRY PELLING. *The British Communist Party: A Historical Profile*. London: Adam and Charles Black; distributed by Humanities Press, Atlantic Highlands, N.J. 1975. Pp. xvi, 204. \$6.75.

Henry Pelling's study of the Communist party of Great Britain first appeared in 1958. The work has now been reissued with his introduction sketching developments since that date, calling attention to recent monographs dealing with the topic. He has refrained from altering the original text, however, thus inviting scrutiny during the era of détente of a work completed amid the tensions of the cold war.

Pelling rejects the party's official view of itself as the left wing of the indigenous British labor movement, perceiving it rather as an alien organization artificially sustained and thoroughly controlled by the Soviet Union. Evidence gained recently both from the disclosures of party leaders and from scholarly studies has tended to support his contention concerning Soviet direction of the party's internal affairs, while calling into question his generalizations concerning the backgrounds of the membership. It now seems fully established that the Soviet Union heavily subsidized the party in the 1920s, and Walter Kendall has documented the skillful utilization of these funds to impose a leadership amenable to Soviet discipline. Lawrence MacFarlane's statistical analysis of the delegates to party conventions, however, refutes Pelling's



assertion that the membership was disproportionately "non-English" in origin.

Though clearly perplexed by the question of why "a band of British citizens could sacrifice themselves so completely over a period of almost forty years to the service of a dictatorship in a foreign country," (p. 191) Pelling strives for a sympathetic treatment of their motives. The exodus of intellectuals from the party in the wake of Khrushchev's revelations and the suppression of the Hungarian uprising demonstrates, he maintains, that they were genuinely deceived—albeit self-deceived—concerning the nature of the Stalinist system.

Since students of the Communist party are forced to rely almost entirely on published materials, any study of the organization is restricted to dealing with internal party debates only in the rare instances when they surface in public. Pelling's work suffers from this unavoidable limitation, but remains, however, the only general history of the party. In the absence of new materials, I doubt that a clearer, more concise, or more objective account will be forthcoming.

CATHERINE ANN CLINE  
Catholic University of America

A. J. SYLVESTER. *Life with Lloyd George: The Diary of A. J. Sylvester, 1931-45*. Edited by COLIN CROSS. New York: Barnes and Noble. 1975. Pp. 351. \$23.50.

A. J. Sylvester served Lloyd George as principal private secretary from 1923 to 1945. Belatedly realizing the historical importance of his position, he began in 1931 to keep a shorthand record of Lloyd George's activities. This diary has now been ably edited by Colin Cross.

Sylvester's diary adds little to our knowledge of English politics during this period. Nor does it reveal any secrets about this last phase of Lloyd George's career, the outlines of which are already well known. However, it contains much interesting new detail illustrating Lloyd George's political decline: his attempts to forge a national coalition in 1935, his misguided admiration for Hitler, his attitudes to the Second World War, and his refusal to serve under Churchill. "I shall wait until Winston is bust," he said.

The main value of the diary lies in its extensive description of Lloyd George's private life, particularly his relations with his family and his mistress, Frances Stevenson. As historical gossip, it is enjoyable reading, with valuable reminiscences by Lloyd George about his career. But it is more than gossip, for the diary presents much information about Lloyd George's personality. Unlike most other politicians, Lloyd George has been judged not just for his political accomplishments, but also

for his moral character, an issue of political importance during his lifetime and of continuing controversy after his death. Most recent academic works have tended to exonerate this side of Lloyd George's reputation. Sylvester's diary will aid both the recent academic supporters and the older critics of Lloyd George. It has many passages that illustrate his personal charm, his dynamic approach to politics, his skill as a talker, and his feelings for his family. But it has striking examples, too, of his childish egotism, autocratic behavior, and lack of moral sensibility. "He just sucks the life out of one," Sylvester laments at one point. Even allowing for the diary's limitations—it describes Lloyd George only as an old man, and Sylvester's viewpoint is ultimately that of a servant who was often treated as such—it is an important source.

PETER WEILER  
Boston College

RITCHIE OVENDALE. *'Appeasement' and the English Speaking World: Britain, the United States, the Dominions, and the Policy of 'Appeasement', 1937-1939*. Cardiff: University of Wales Press. 1975. Pp. v, 353. £9.00.

MAURICE COWLING. *The Impact of Hitler: British Politics and British Policy, 1933-1940*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1975. Pp. x, 561. \$42.50.

The 1930s have become the focus of a growing literature. That decade may yet hold the same fascination for historians that World War I has had for a generation of scholars. Ritchie Ovendale's book is a pioneer study of the Dominions and appeasement, while Maurice Cowling's is in the revisionist tradition of A. J. P. Taylor, Donald Lammers, Martin Gilbert, W. N. Medlicott, and D. C. Watt.

Ovendale demonstrates the impact of American and Dominion public opinion upon the policy of appeasement during 1937-39. American isolationism and the Imperial Conference of 1937 did not so much induce as reaffirm Chamberlain's view that appeasement was the right policy for preserving European peace. President Roosevelt issued repeated appeals for the preservation of world peace while all of the Dominions, except New Zealand (Ireland is not considered), opposed any war over Central or Eastern European issues. Both Australia and South Africa argued that, for ethnic reasons, the Sudetenland belonged within the German Reich. Ovendale believes that American and Dominion attitudes exercised some influence on Chamberlain's deliberations during the Munich

crisis, but that neither were notably decisive factors in the formulation of British policy during the period immediately preceding the outbreak of war. What perhaps needs some qualification is the author's presentation of the Chamberlain-Eden dispute: the former is seen as expecting little more than rhetorical proclamations from the United States and believing that British foreign policy should be directed toward some lasting accommodation with the dictators; Eden is portrayed as deeming any such working relationship with the Fascist powers impossible and wishing to devote Britain's efforts to securing the cooperation of the United States despite that nation's isolationist proclivities. The Chamberlain-Eden differences ended in Eden's resignation, but there is persuasive evidence that the foreign secretary objected not so much to the policy of appeasement as to its timing, particularly as it applied to the Italians.

For his analysis of Dominion policy, Ovendale depends upon parliamentary debates, published memoirs, and newspapers. He did not gain access to the Hertzog and Smuts papers in South Africa or to the Chamberlain papers. American sources are even more limited and include only memoirs and published secondary materials. The study's strength derives largely from the excellent use that Ovendale makes of the British cabinet and Foreign Office papers. He succeeds in showing that "it was largely Chamberlain's policy of appeasement which ensured that when war came in 1939 the Commonwealth was united. Britain, too, had behind it a sympathetic United States, and Roosevelt's assurance that the industrial resources of his country would be at Britain's disposal. This was a considerable achievement."

Chamberlain emerges as the dominant personality in Maurice Cowling's work also; he is portrayed as the successful domestic politician who so often was both right and righteous. Because he made foreign policy a personal policy, Chamberlain, the peerless parliamentarian and accomplished social reformer, destroyed himself and thereby helped discredit all Tory libertarian efforts of the 1930s. It may be true that Halifax represented "the embodiment of Conservative wisdom who decided that Hitler must be obstructed because Labour could not otherwise be resisted." And Churchill and Eden doubtless went farther along the road with Chamberlain than is admitted in their autobiographies. But in reassessing the villains and the heroes of the more traditional interpretations of this period, Cowling does not always exercise scrupulous detachment. Is there conclusive evidence to support the notion that Halifax, motivated by party loyalty, served as the instrument of Chamberlain's downfall? Even less

convincing is the depiction of Churchill as the jingoistic alcoholic who embraced the antidictator crusade to get back to the "centre of the scene."

Cowling's book, a prodigious piece of research, includes copious notes, biographical entries, and an impressive bibliography. The nonspecialist will find the narrative occasionally bewildering owing to the skillful, but often distracting, mixing of political cameos and anecdotal details. Insisting that it was "neither morally obligatory nor prudentially self-evident that Hitler should be obstructed in Eastern Europe," Cowling's book promotes a controversial thesis and provides an insightful and stimulating analysis of the impact of Hitler upon "high politics" in Britain.

THOMAS E. HACHEY  
Marquette University

DANIEL WALEY. *British Public Opinion and the Abyssinian War, 1935-6*. London: Maurice Temple Smith, in association with the London School of Economics and Political Science. 1975. Pp. 176. £6.00.

Daniel Waley's subject matter is public opinion at a time "shortly before the introduction of opinion polls in Britain" (p. 11). Newspapers accordingly serve as sources when they provide information on opinion. Other sources are diaries, memoirs, pamphlets, correspondence and the direct testimony of survivors, cabinet papers, the League of Nations Union's papers, and the minute books of some of its branches.

The events and policies to which opinion responded make a familiar story told here with easy economy: the peace ballot, Italy's invasion of Abyssinia, and the November 1935 general election, which compelled a reluctant Baldwin to support the League and sanctions for collective security. There soon followed the Hoare-Laval Pact, which was aborted by the vehemence of governmental and public criticism. After Mussolini's rapid victory, the ineffective sanctions were lifted, although the League of Nations Union demanded that it be maintained to vindicate law and morality.

The author finds public opinion influential on political pronouncements, not on policy. His probings concerning the volume of public mail to MP's provide evidence that the volume was much less than can sustain the story of an aroused nation directing its government.

Waley's judgment is painstaking and discriminating. He has a flair for good quotations and telling asides. For all his merits, he does not wholly avoid the tedium and repetition of public opinion studies.

M. A. FITZSIMONS  
University of Notre Dame



WILLIAM THORNHILL, editor. *The Modernization of British Government*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. ix, 322. \$21.50.

Britain was the only major West European country to survive the Second World War with its central political institutions intact. It is understandable, therefore, that immediate postwar British politics were devoted more to social reform than to a critical scrutiny of the "Westminster Model," which was believed to be suitable not only for Britain herself, but also for the new Commonwealth countries of Africa and Asia.

The last ten years have, however, been years of institutional change; the civil service has been reformed, following the recommendations of the Fulton Committee; the local government system has been restructured; Britain has undergone its first national referendum, over the renegotiated terms of entry to the EEC; and there have been continuing experiments with the reform of parliament and central government. The parties are currently grappling with the problems of devolution of power to Scotland and Wales, problems that raise complex and crucial constitutional issues.

A book that succeeded in analyzing these changes would, therefore, be of immense value. Lord Armstrong, the former head of the civil service, in his foreword to this book comments on the need to chronicle recent changes "so that we can all look back with some historical perspective on what we have lived through the better to understand why the various institutions have the shape they have today"; and Thornhill in his introduction expresses the hope that overseas readers will "find that the book provides them with a clear and overall picture of the manner in which British Government has evolved."

Unfortunately, these aspirations are hardly realized, and the book rarely rises above the level of a dull catalog of events. The contributors seem too concerned with the form of institutions, and not enough with their working. In their concern with the minutiae of machinery, they fail to consider what animates the structure and gives it life. One of the reasons for the "official" nature of much of this book is that a number of the chapters are contributed by former civil servants, too closely involved in the changes they describe to be able to see them in a wider perspective. Another reason, perhaps, is the closed and secretive nature of British institutions, so alien to the spirit of government in the United States.

It is a pity that there is no discussion of what the reforms were trying to achieve and whether they succeeded, and whether the process of reform via the instrument of the Royal Commission reveals

anything about the nature of British government. The absence of a discussion of devolution and regionalism is also a serious weakness in a book of this nature.

*The Modernization of British Government* cannot, therefore, be very highly recommended; anyone seeking to acquire a balanced account of recent changes would do better to consult Frank Stacey's *British Government, 1966-1975: Ten Years of Reform* (Oxford University Press [1975]).

VERNON BOGDANOR  
Brasenose College, Oxford

ARCHIBALD A. M. DUNCAN. *Scotland: The Making of the Kingdom*. (The Edinburgh History of Scotland, volume 1.) New York: Barnes and Noble. 1975. Pp. xii, 705. \$35.00.

The publication of this book, volume 1 of the four-volume Edinburgh History of Scotland, is a notable event in Scottish historiography: the first specialist survey of Scotland from prehistory to 1286 to be published since 1862. The completion of this Edinburgh project—modeled on the Oxford History of England—should be gratifying to Scots who do not like to see their country ignored and to scholars distressed by the proliferation in recent years of popular and emotion-laden general histories of Scotland. These four volumes by distinguished historians are eminently weighty, and they avoid that maudlin self-congratulation which national histories are heir to; a charming exception is Duncan's confirmation that medieval Europe's ideal lover, Tristan, was a Pict.

The author's coverage of the first seven millennia of Scottish history (beginning in the north of Fife) is necessarily sketchy. In the first hundred pages, he surveys Bronze Age and Roman Scotland, the incursions and amalgamations of the Celts, the invasions and settlements of the Vikings, and the ravaging of Anglian lands. The narrative pace is fast and occasionally restless. Duncan is all too aware not only of the immense darknesses of prehistory, but also of the foibles of modern historical scholarship (the 1962 census of Neolithic Scotland, for example, decrees that the population "can hardly have exceeded two people for each of the modern counties"). One gets the impression that the author is hastening to reach the late eleventh, twelfth, and thirteenth centuries, to which the remaining five hundred pages of text are devoted. The chronological political narrative takes us from the reign of Malcolm III (of Macbeth fame) to Alexander III's fatal accident in 1286, which led to the wars of independence. The slower pace allows for more contemplation, more analysis of original sources, and more synthesis. Successive

chapters illustrate the making of the kingdom: how Scotland acquired a monarchy, a feudal organization, firm boundaries, a body of law and custom, urban communities, commerce, and a Church whose dioceses and parishes were filled by the religious orders of medieval Christendom. The author presents us with a balanced rendering of political, economic, and social accomplishments. Those wishing for more on cultural or intellectual matters must heed Duncan's warning that Scotland before 1300 had no great thinkers, no great men of spirituality, and no great intellectual centers. The one scholar of originality, Duns Scotus (d. 1308), fled his native soil to win fame abroad.

Duncan's volume is by far the best overview of the period now available and particularly useful for the twelfth and thirteenth centuries: the narrative is clear, the prose impeccable, the author's judgments sound, and his erudition spiced with wit. It will be the standard history of early medieval Scotland for years to come.

ROBERTA FRANK  
University of Toronto

ARTHUR MITCHELL. *Labour in Irish Politics, 1890-1930: The Irish Labour Movement in An Age of Revolution*. New York: Barnes & Noble. 1974. Pp. 317. \$10.75.

Arthur Mitchell's work is a survey of the Irish labor movement and its role in Irish politics. He outlines the establishment of the Irish Labour Party in 1912, as founded by the Irish Trade Union Congress, and its subsequent problems and achievements. Even before the creation of the party, the Congress had decided that the organized labor movement should not take a position on the broad subject of Irish nationalism or the narrower subject of home rule. These were but the first of many difficulties standing in the way of the party in its constant struggle to achieve recognition and credibility.

The Irish Labour Party, like the Trade Congress, straddled the issue of Home Rule. During World War I Irish labor leaders were critical of involvement in the war, and no Irish-based labor organization supported the British position. In 1915 the Labour Party did not advocate socialism or revolution but stressed the need for public housing, social services, and educational reforms. The next year labor was active in neither the Citizen Army nor the Easter Rising.

During the struggle for independence, the party did not take a strong position but acted primarily in a subordinate role to that of the Sinn Féin leadership. Likewise, the party did not enter into the discussion of the future government of Ireland, hoping that this problem would be settled by others. The labor movement did, however, initiate

several strikes in Ireland during 1919 and 1920 that helped to undermine British authority.

A number of reasons contributed to the failure of the Labour Party to achieve a strong position in elections. It lacked sufficient money to enter a large number of candidates; it was narrowly based, not appealing to small farmers and farm laborers; it never became the focus of radical or progressive activity; it gained only a small industrial vote, especially when industrial Belfast became separated; and its platform was more social-economic than political in nature.

The study is based upon extensive research in periodicals and labor reports, as well as on personal interviews. It provides extensive data on labor's role, or lack of role, in Ireland during the period covered.

HOMER L. CALKIN  
Department of State

JOSEPH T. CARROLL. *Ireland in the War Years*. New York: Crane, Russak and Company. 1975. Pp. 190. \$10.75.

This book is a readable and concise story of the diplomacy of the Republic of Ireland (then styled Eire or Ireland) in maintaining its neutrality during World War II. It is largely, and necessarily, the story of Eamon de Valera, since he was both prime minister and foreign minister of Eire during those years.

Neutrality presented few problems until the fall of France gave control of Europe's Atlantic coast to Nazi Germany; henceforth, German submarines endangered British lines of communication with North America. During the autumn and winter of 1940 Churchill exerted heavy pressure on de Valera to grant port facilities to the British navy but was met by the latter's determination to remain neutral. De Valera convinced both Allied and Axis governments that any infringement of Irish neutrality would be met by armed resistance. Fortunately, the British position never became desperate (partly owing to the presence of British bases in Northern Ireland which was part of the United Kingdom) so that Ireland was able to remain neutral.

Problems arose from British and American fears that Axis diplomatic representatives in Dublin were sending military information to their governments, that German submarines were being refueled off the Irish coast, and that German agents were finding useful friends in the I.R.A. These fears were either partly or wholly unjustified, and owed much to the fertile imagination of David Gray, the American minister to Eire.

Joseph T. Carroll rightly sees the preservation of Irish neutrality as due to luck, to de Valera's supe-

rior diplomatic sense, and to the fact that Irish neutrality favored the British war effort in recruits to the British armed forces and to British factories.

This book will remain useful despite the opening of Irish cabinet documents since its publication. A preliminary examination of these documents suggests that they add little to our previous knowledge.

MAURICE R. O'CONNELL  
Fordham University

ROBERT GUILLOT. *Le procès de Jacques Cœur (1451-1457)*. Paris: Éditions de la C.N.M.H.S. 1975. Pp. 165.

There have been so many books about the career of the master of the king's mint and the steward of royal expenditure to Charles VII that it might be difficult to justify another book about Jacques Cœur. But romanticism and popular legend have abounded, and the truth about the trial of the merchant of Bourges who rose so rapidly and fell so abruptly from royal favor has been elusive. Previous studies have been based on a few known documents and have been so fragmented that it has been impossible to assess critically Jacques Cœur's career, and especially the mysterious events pertaining to his fall from royal favor, arrest, trial, and condemnation.

Robert Guillot's contributions have been to collect the numerous documents relating to the trial—from the libraries and archives in Paris to the Bibliothèque Municipale de Roanne—and then to subject them to critical analysis within the text. Some of the documents such as the *Journal du Procureur Dauvet*, having been previously published, are familiar to scholars and are therefore referred to by Guillot only for the purpose of relating them to recently uncovered manuscripts. Others, such as legal documents from the archives of Roanne, have been recently "discovered" by Guillot, which justifies their being completely transcribed here.

Guillot focuses on the collection of documents, which he allows to speak for themselves, relating to the *procès*. In valuable footnotes he refers these documents to others that are published and unpublished. The critical examination of the documents of the trial and condemnation of Cœur is this book's most outstanding contribution, one that is enhanced by a brief reiteration of the facts of Cœur's life, including the events of the trial, and the addition of maps, illustrations, a useful set of indexes, and a bibliography of all the known manuscripts relating to Cœur's career. Guillot has presented us with a case study of fifteenth-century French law and legal institutions, especially as they were emerging in cases of *lèse majesté*. The official application of inquisitorial procedures that

permitted the confiscation of the property of those condemned appears here as a step in the development of royal legal strategy. In addition, new historical insights regarding Jacques Cœur the criminal and Jacques Cœur the victim will undoubtedly emerge as a result of this thorough textual study.

CHARMARIE JENKINS BLAISDELL  
Northeastern University

J. H. M. SALMON. *Society in Crisis: France in the Sixteenth Century*. New York: St. Martin's Press. 1976. Pp. 383. \$19.95.

J. H. M. Salmon's *Society in Crisis* meets a long-standing need for a thorough and up-to-date history of sixteenth-century France and, in particular, of the religious wars. The essential contribution of the book is to put the well-known story of those wars in the broader context of social and institutional change. Salmon does not attempt a "total history," and, when compared to the more *outré* work of contemporary social historians, his approach may appear a bit old-fashioned. What he does offer is a clear and balanced analysis of those fundamental shifts in state and society that preceded the civil strife and that were, in turn, intensified by the sixteenth-century crisis.

It is Salmon's central argument that the characteristic institutions, social relations, and political mentality of the Old Regime were molded during the course of the sixteenth century. He emphasizes especially the development of a professional administrative cadre within the monarchy and of a powerful order of venal officeholders that would serve as a counterpoint to the traditional military aristocracy. To defend his general proposition, Salmon revives an older line of interpretation that stressed the fairly steady growth of authoritarian monarchy from the late fifteenth century. In his view, aristocratic clientage and venality of office, which some would interpret as signs of a devolution of royal authority, tended ultimately to enhance the independent power of the crown. Finally, Salmon contests the thesis that sixteenth-century monarchy was consultative in character and different in kind from the absolutism that took root after Henry IV came to the throne.

On such fundamental problems as the relations of social class and Calvinism, the changing patterns of landholding, popular revolts, and urban discontents, Salmon provides illuminating and nuanced interpretations. However, the larger vision of state and society that imbues his work is overly schematic and rather too dependent on a one-sided reading of the significance of the *pauvette* of 1604. Having disposed of one "model" of monarchy, he appears to outline another in which the

crown derives strength from a new social balance or duality between "sword and gown." Whatever the abstract truth of this conception, it represents a very simplified view of the complex relations between the early modern French monarchy and the many diverse elites that composed the nobility. But synthesis entails simplification, and this book is, for the most part, a superior synthesis that nicely combines social and economic history with a good, if somewhat dense, account of the religious wars.

RAYMOND F. KIERSTEAD  
Catholic University of America

JEAN BODIN. *Colloquium of the Seven about Secrets of the Sublime*. Translated, with an introduction, annotations, and critical readings by MARION LEATHERS DANIELS KUNTZ. Princeton: Princeton University Press. 1975. Pp. lxxxii, 509. \$25.00.

Jean Bodin (c. 1529–96), a French philosopher, savant, and judge, was truly a *uomo universale*, a renaissance man in his catholic interests. He was a student of classical Greek, Latin, and Hebrew; the formulator of the modern theory of sovereignty and of modern absolutism; a writer on witchcraft; a student of the Cabala and other esoteric wisdom; and an advocate of religious toleration and an associate of French *politiques*. His writings reveal his concern with the true basis of human existence: it was during the last sixteen years of his life that Bodin—in spite of the fluctuating ups and downs of career and fortune during this violent phase of the French civil wars—composed three works that show his acutely religious and philosophical nature. *De la démonomie des sorciers* (1580) explores the metaphysical realm of spirits and demons as agents of God's plan. *Universae naturae theatrum* (1596) presents a dialogue on natural science and the universal nature of God's plan. Finally, the manuscript *Colloquium Heptaplomeres*, a dialogue among men of seven different faiths, is the most revealing of all his writings. Since it was boldly critical of much thought essential to sixteenth-century religiosity and seemed to profess a true ecumenical spirit encompassing all faiths, it could not be printed, but it was widely circulated in manuscript.

Marion Kuntz's translation, with a critical introduction, annotations, and variant readings, is here in its first English edition, only preceded by Noack (1857, Latin), Guhrauer (1841, German/Latin), and Chauviré (1914, French extracts). It is in keeping with modern ways of scholarship that footnotes and source quotations are as brief as possible; dealing with a work as diffuse and difficult as the *Heptaplomeres*, I, at least, prefer

the fuller documentation and explanations found in the earlier editions.

I am pleased that Marion Kuntz, in the introductory essays, outlines Bodin's use of classical and mystical materials and ideas. When the French Cabalist Guillaume Postel died in 1581, he left his extensive library to his friend Jean Bodin. And Postel's use of Cabalistic concepts is reproduced in the *Heptaplomeres* (compare the introduction, liii–lxii). Did the Jew Salomon express Bodin's ideas in this field?

The translation of the difficult and frequently turgid Latin of the manuscript has been well done. I am pleased that this sixteenth-century *opus maleficum*, which argued so convincingly for religious toleration, is now available in an unabridged English version.

FRANK ROSENTHAL  
University of Judaism

RENÉ PILLORGET. *Les mouvements insurrectionnels de Provence entre 1596 et 1715*. Paris: Editions A. Pedone. 1975. Pp. 1044.

The brilliant burst of scholarship on Provence during the *ancien régime* begun sixty years ago by Paul Masson, Adolphe Crémieux, and Raoul Busquet has been continued in recent years by Charles Carrière, Edouard Baratier, Maurice Agulhon, André Bourde, Michel Vovelle—and now René Pillorget who has investigated revolts in this southeastern French province from 1596 to 1715. Pillorget's long-awaited *thèse d'état* for the University of Paris is the product of twenty-five years' research in local and national archives. Demonstrating a high level of scholarship, Pillorget is unusually comprehensive and meticulously accurate: the book's sheer size is impressive. Pillorget's account of the Fronde in Provence fills a significant gap in the literature; in fact, one wishes it were longer and the book's focal point. His descriptions of municipal politics and government in Provence provide new information on a long-neglected subject.

Pillorget's main concern has been to classify types and subtypes of revolts. He divides the seventeenth century into five time zones: 1596–1635, 1635–1647, 1648–1653, 1653–1661, and 1661–1715; and he groups revolts in these zones by participants and causes in a complex, confusing hierarchy of types. Revolts in the community occurred within the elite, between the elite and popular classes, and among political factions, while community revolts erupted against outside authority: seigneurs, soldiers, foreigners, royal agents, and the crown. Causes were primarily socioeconomic. The revolts were sporadic in occurrence and un-

even in importance, and there was surprisingly little violence. The decades after 1661 were calmer.

Pillorget's models of revolts are not as original as those of his colleague, Yves-Marie Bercé, who recently published a study of revolts in seventeenth-century Aquitaine, but Pillorget explores the role of the provincial elite, particularly officials, who were neglected by Bercé. Preoccupied by classifying events and constructing complex socioeconomic models, Pillorget unfortunately does not explain the significance of reoccurring political issues or delve into the motivations of elite leaders of the revolts. Less classification and more analysis of politics and personalities might have given a greater sense of continuity between revolts. Pillorget relies too heavily for biographical information on the often erroneous *Dictionnaire biographique* of the *Encyclopédie départementale Bouches-du-Rhône*—one expects more from such extensive local research—and he does not fully explore the political importance of clientage ties. Despite its title, the book concentrates on revolts at Aix, Marseille, and Draguignan, the most rebellious cities; revolts elsewhere in Provence are seen from this somewhat distorted vantage point. Finally, the book suffers from a common flaw of the *Annalistes*: massive, overwhelming documentation for its own sake. Nonetheless, Pillorget has produced an important study on seventeenth-century revolts, adding much new information on Provence. His book should be consulted by all serious students of the *ancien régime*.

SHARON KETTERING  
Montgomery College

J. MICHAEL HAYDEN. *France and the Estates General of 1614*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press, 1974. Pp. xii, 334. \$17.50.

J. Michael Hayden's monograph seeks to place the history of the Estates General of 1614 in the broader context of French political history during the regency of Marie de Medici. The book provides a useful account and analysis of the rancorous Estates, but it will be best read as a revisionist essay on the rule of the second Medici queen. What Hayden clearly demonstrates is the continuity between the policies of Henry IV and those of his wife—a continuity personified in Marie's crown servants, such as Villeroy, Sillery, and Jeannin, whose service to the monarchy began in the tumultuous days of the later religious wars. In this respect Hayden's study is of a piece with other recent work that has partially rehabilitated

both Catherine and Marie de Medici and has treated the two queens as something other than alien grafts upon the French body politic.

Hayden also undertakes to rehabilitate the reputation of the Estates General, and, given the fractiousness and impotence of the Estates, this is not an easy task. Four chapters detail the wrangles among the orders, within the orders, and between the orders and the crown. A final chapter concludes that the Estates General offered the country a unified and prudent program of reform. Yet, as Hayden recognizes, the dead hand of the past dominated the meetings, the debates were often sterile, and, in the end, the *cahiers* of the three orders were ignored. In light of this and other modern studies of representative institutions in sixteenth- and seventeenth-century France, one must conclude that the vital centers of French political and social life lay elsewhere.

This is a meticulous work, supported by numerous useful charts and a full list of the delegates to the Estates General. It is also a straightforward study that avoids the discussion of typology that has characterized much of the writing about representative institutions in early modern France. Hayden treats the meeting of the Estates General as part of a thoroughly unheroic royal policy "to hang on until better days." He has put the Estates General in its proper perspective.

RAYMOND F. KIERSTEAD  
Catholic University of America

CISSIE C. FAIRCHILDS. *Poverty and Charity in Aix-en-Provence, 1640-1789*. (Johns Hopkins University Studies in Historical and Political Science, series 94, number 1.) Baltimore: Johns Hopkins University Press, 1976. Pp. xii, 197. \$12.95.

The gamut of charitable institutions in the capital of Provence affords the subject matter of Cissie Fairchild's fresh, engaging study. The oldest was a thirteenth-century leprose; the largest before 1600 was the Hôtel-Dieu St. Jacques. Robert Mandrou in 1967 praised Mme. Sabatier-Duc's massive unpublished study of St. Jacques as a model for the exploitation of hospital archives in cultural and social history. Fairchild has made use of that study, but draws to a far greater extent upon the archives of La Charité, founded in 1640 in order to distribute bread and to house orphans, the aged, and the unemployed. It was the intended crown of Aix's charities, all but four of which derived their capital and organization from an outpouring of religious zeal in the early decades of the seventeenth century.

Fairchild stresses the charities' diverse community roles. A succession of social elites derived



status from governing them; they functioned in the town as employers, consumers, and bankers. Investment in *rentes* anchored their otherwise contentious assets. Forced to cover deficits, they in turn issued *rentes*, often in small amounts, to bourgeois, craftsmen, and even to inmates of charitable houses. Recipients of charity were generally natives of the town, Catholic, and victims of "structural" poverty. About twenty percent of Aix's population received some type of aid. The poor outside the charities—those subject to repression as beggars, vagrants, or criminals—tended to be outsiders or victims of a "conjunctural" poverty.

Socioeconomic pressures set the stage for a shift in religious mentalities and created, around 1760, "the crisis of traditional charity." Here, Fairchilds links (perhaps too neatly) the studies of Baehrel and Vovelle to data on the decline in charitable donations. Royal control stabilized hospital finance at a high cost to the poor. Royal efforts to confront mendicity outside the charities were bungled and obstructed. Nevertheless, the perception of poverty as an economic phenomenon, beginning about 1760, paved the way for a painful but much-needed transition to a national system of *bienfaisance*.

Fairchilds' sparsely written text makes room for well-chosen detail: tabulations of occupational origins and physical disabilities of hospital inmates, the record of heated wrangles over bequests, a stark list of an inmate's wordly goods, and more. There are some weak points. The "jump" in the incidence of begging is problematic (p. 133: the figure quoted for 1773 is a cumulative total and, unlike the yearly average given for an earlier period, includes the whole *intendance*); some of Laverdy's directives are ascribed to Bertin (p. 149). The linkages from town to province to kingdom need more comment. Parisian sources would tell more about the reform commission of 1761, the politics of the Estates, and the activity of Boisgelin, archbishop of Aix—who in 1789 reminded the National Assembly that many a debt-laden hospital had survived only by "the gifts of good citizens."

THOMAS M. ADAMS  
Ottawa University,  
Kansas

ANNE SAUVY. *Livres saisis à Paris entre 1678 et 1701*. (International Archives of the History of Ideas, number 50.) The Hague: Martinus Nijhoff. 1972. Pp. 430.

Because it witnessed the origins of the Enlightenment and climax of French absolutism, the period 1678–1701 is particularly fruitful for studying both the diffusion and repression of ideas. Anne Sauvy's

analytical bibliography of the materials in the Bibliothèque Nationale's Ms. Fr. 21743 confirms this fact anew. The document itself is a fifty-two-page folio manuscript drawn up in 1701 by the Paris Community of Bookdealers and Printers. It lists the titles of 1,115 works seized in the capital over the previous twenty-three years. Sauvy does not reconstruct the provenance of the document, but her able introduction summarizes the character of the appropriated volumes: forbidden books, books printed without royal privilege, counterfeit editions, and respectable works found by the police with ones illegally printed. In the descents starkly reported by Ms. 21743, Sauvy estimates that thirty thousand pieces of merchandise were rounded up. The number of seized copies of each book varied from a single one to six thousand. More than half the titles were of a religious character—largely Protestant, Quietist, and Jansenist works. Seized on fifteen different occasions, Courtilz de Sandras' *Vie de Turenne* was the book best known by the police; right behind, however, were Protestant Bibles with fourteen seizures, Pascal's *Provinciales* with thirteen, Gui Patin's *Lettres* and Bayle's *Nouvelles de la République des Lettres* with eleven apiece, and Petitdidier's *Apologie des Lettres provinciales* with ten.

Sauvy reproduces Ms. 21743, with its bare lists of titles. She then constructs an analytical bibliography in which she includes probable authors, acknowledged (though not necessarily veritable) dates and places of publication, asserted publishers, and the known publication history of each title. Time and again the mythical Pierre Marteau of Cologne appears on title pages, and one wonders how many hundreds of volumes so credited were derived from presses in Lyon and Rouen that were denied legitimate work by the pernicious privilege monopolies of the *chambre syndicale* of Paris. Sauvy's catalog is a mine of information, and her erudition is thoroughly unobtrusive. We learn that the French translation of Spinoza's *Tractatus theologico-politicus* was published under four different titles in order to elude censors; a number of books eventually confiscated found an elegant binding of "maroquin du roi" and entered the Bibliothèque Royale. For the royal councilors, *parlementaires*, Sorbonne professors, and bishops who initiated the descents, Rabelais, La Rochefoucauld, La Fontaine, and Richard Simon shared notoriety with the authors of *Vénus dans le cloître*, *La putain errante*, and *La religieuse cavalier*. Bookburners of any age rarely make qualitative distinctions. They nevertheless make a mark in the social history of ideas, and Sauvy's fine scholarship has uncovered victims of their handiwork.

RAYMOND BIRN  
University of Oregon



ROGER PRICE. *The Economic Modernisation of France*. A Halsted Press Book. New York: John Wiley and Sons. 1975. Pp. 235. \$17.95.

Roger Price begins his synthesis of the recent research in eighteenth- and nineteenth-century French economic history by rejecting conventional periodizations and by de-emphasizing politics as a factor in economic development. In contrast to similar works—notably those of Tom Kemp and Milward and Saul—that identify the French Revolution and the Napoleonic empire as an economic and political watershed, Price denies the social and economic importance of the events of 1789–1815. In his view the *ancien régime* continued economically until the advent of railroads transformed the market structure of France by destroying compartmentalized regional markets and by creating a single national market in the 1850s. For Price this is the key event in the process of modernization whereby the preindustrial French economy, after developing to its natural limits in the first half of the nineteenth century, gave way to the emerging industrial economy between 1850 and 1880.

Certainly there is much to recommend this interpretation. Indeed, it would be hard to overestimate the impact of the railroad on the economy of any European country in the nineteenth century. But, in Price's hands, the argument is somewhat one-sided. While stressing the railroads' influence on agriculture, industry, commerce, banking, and population, he neglects to explore the process by which the railroads came into being in the first place. As a result, we get a good picture of how the structure of the French economy was changing in the mid-nineteenth century, but only a partial explanation of why it was changing.

Since the book will likely be considered for classroom use, it is only fair to note that it is not particularly well written or well edited. The text suffers from too many one-sentence paragraphs and from a lack of continuity between paragraphs. Some passages are incomprehensible (note the opening sentence of chapter 2), and some are ungrammatical (see the first sentence under "Capital Investment," p. 141). Typographical errors crop up with irritating frequency. On the other hand, the theme is clear and consistent, and there are extensive bibliographies accompanying each chapter.

In sum, the book may not be the definitive work on French economic modernization, but as an introduction to the subject its merits generally outweigh its faults. It is unfortunate that the extent of its use may be limited by an exogenous factor, the price. At \$17.95 for 226 pages of text, notes, and bibliography, it is overpriced by any standards.

The impecunious—that is, most faculty and students—can only hope that a paperback version will be forthcoming.

MICHAEL S. SMITH  
*University of South Carolina*

MICHEL VOVELLE. *Piété baroque et déchristianisation en Provence au XVIII<sup>e</sup> siècle: Les attitudes devant la mort d'après les clauses des testaments*. (Civilisations et mentalités.) Paris: Plon. 1973. Pp. 697.

This pioneering study of the changing attitudes toward death in Provence in the eighteenth century has not received the widespread recognition in American historical circles that it merits. A masterful example of quantitative social history based on an analysis of a sample of some two thousand wills found in departmental archives, Michel Vovelle's work typifies the best of the new school of religious studies in France, a school that has abandoned the classical narrative account to investigate the religious experience in all its manifold dimensions. Rather awkwardly labeled as "historical religious sociology," this pluralistic approach explores the labyrinths of popular religion, plumbs collective mentalities, and weighs the psychology of the religious experience.

Vovelle adds important nuances to the thesis of Maurice Agulhon (*La sociabilité méridionale*, 2 vols., Aix-en-Provence [1966]) that a profound mutation occurred in the *sensibilité collective* in Provence, a mutation toward laicization. The touchstone for charting this shift in collective mentality is the behavior of notables at that crucial time in their lives when they were writing their wills. Vovelle notes a profound change in their attitude toward death as reflected in the decreasing concern with the "baroque" rituals of elaborate funerals, legacies for perpetual masses, and burial in particular holy places. In this individualization of attitudes toward their funeral trappings Vovelle sees the signs of a crumbling of the society of orders, with its emphasis upon the observance of death in a fashion prescribed by the deceased's rank. Signs of this change are seen in a decline in the weight of candles stipulated to be lighted, a decrease in invocation of the Virgin, and a trend toward informality in clothing the body.

In a broader context this book gives support to the thesis that the dechristianization movement is not to be dated from the onset of the Revolution; instead, if defined as a partial detachment from ecclesiastical traditions, the movement clearly begins earlier in the age of the Enlightenment. Yet, anticipating the nineteenth century, one is reminded that the quantitative decline in adhesion to the Catholic cult was accompanied by a qualitative purification within the Church, a phenome-

non of equal interest to specialists in the early modern period.

WILLIAM H. WILLIAMS  
University of Maryland,  
College Park

CLAUDE C. STURGILL. *Claude Le Blanc: Civil Servant of the King*. Gainesville: University Presses of Florida. 1976. Pp. v, 202. \$8.50.

This book is Claude C. Sturgill's second attempt to "refine the existing information on the French army during its critical years." It serves as a pendant to his *Marshal Villars and the War of the Spanish Succession* (1965). *Claude Le Blanc* describes the public career of the man who served as intendant in Auvergne (1704-08) and then was transferred to Flanders, a chaotic frontier province in the throes of the worst setbacks of the War of the Spanish Succession (1708-15). He came to power in 1715 as an ally of the Regent Orléans and John Law, becoming a member of the War Council of the *Poly-synodie* (1715-18), and he ultimately served as secretary of state for war (1718-23, 1726-28).

Sturgill has undertaken a serious but very narrowly conceived search through the ministerial and military archives, producing a modest study that reflects both the profusion of detail and the restricted, administrative point of view of his sources. More than three-fourths of the text is devoted to the war period in Flanders. Readers will find useful specifics about military espionage, relations between Versailles and the war zone, the handling of the extreme conditions of 1709-10, and especially the delicate maneuvers by which the French stalled the English demolition of the fortifications of Dunkirk until they could build a rival canal and port at nearby Mardyck. They will regret the lack of discussion of Le Blanc's family and fortune, his intriguing connections to the coterie of financiers and munitioneers around the Bazin de Bezons family and the regent, or the effect of his administration on the population. Civilian agencies are discussed very little, and the terms used to analyze them are misleading, especially in the reform of the "provincial police" of 1720.

WILLIAM H. BEIK  
Northern Illinois University

ALAIN LE BIHAN. *Frans-maçons et ateliers parisiens de la Grande Loge de France au XVIII<sup>e</sup> siècle (1760-1795)*. (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, number 28.) Paris: Bibliothèque Nationale. 1973. Pp. 509.

AIMÉ COIFFARD. *La vente des biens nationaux dans le district de Grasse (1790-1815)*. Preface by ROGER AUBENAS. (Ministère de l'éducation nationale, Commission d'histoire économique et sociale de la Révolution française. Collection de documents inédits sur l'histoire économique de la Révolution française. Département des Alpes-Maritimes.) Paris: Bibliothèque Nationale. 1973. Pp. 220.

Over one-half of each of these books is a meticulous and careful collection of documents. Aimé Coiffard, in his examination of a district in southern France, has assembled a complete list of the sales and purchases of national lands in Grasse from 1789 to 1815. Alain Le Bihan has gathered together a more diffuse set of papers whose focus is freemasonry in eighteenth-century Paris. Having previously published two other collections of material on freemasonry, Le Bihan here attempts to round out his contribution. In 1773 a schism in the ranks of freemasonry produced two national organizations. In this volume, Le Bihan turns his attention to the lesser of these two groups, *La Grande Loge de France*, and to the fringe masonic orders that clustered around and annoyed the enfeebled *Grande Loge*. For these associations, Le Bihan has assembled a wealth of biographical data on the membership, and he has reproduced a mass of information on their organization and daily activities.

Although students of Old Regime freemasonry will doubtless rely on Le Bihan's documents, they will find little in his introduction, for he has confined himself to a narrative history of the internal politics of the *Grande Loge*. He makes no reference to the social, recreational, and educational functions of masonry, either for the participants or for the larger community. In addition, Le Bihan's description is virtually without analysis. For example, his discussion of the schism of 1773 relates in detail the machinations of the opposing factions, but it ignores the motivations of each side. Le Bihan's narrow emphasis on the chain of political events leaves his exploration of the masons shallow, lifeless, and insubstantial.

In contrast, Coiffard's introductory essay ably illustrates the potential effectiveness of a collection of documents. Here the author places the distribution of national lands within the social fabric of Grasse and tries to assess the financial and social ramifications of these sales. His study generally substantiates the accepted notions about this topic: the merchants and the *négociants* bought most of the land while a number of smaller landholders were able to add slivers to their tiny holdings. Coiffard has nonetheless contributed a summary of the data, successfully verifying the standard theses on this important concern. Fur-

thermore, his precisely focused study begins to uncover the human aspect of the sale of national lands. This microscopic view of the activities of the purchasers reveals, behind the cold statistics, the strenuous efforts of the clergy, the emigrés, the suspects, and the villages that owned communal property to retain their land.

JACK R. CENSER  
College of Charleston

MARC MARTIN. *Les origines de la presse militaire en France à la fin de l'Ancien Régime & sous la Révolution (1770-1799)*. Château de Vincennes: Ministère de la Défense, État Major de l'Armée de Terre, Service Historique. 1975. Pp. 424.

It has long been known that a "military revolution" accompanied the Atlantic Revolution of the late eighteenth century. The dawn of total war was one of the most important events of the modern world. Until recent research by John Shy, David Bien, and Samuel Scott, among others, the complexity of the transition from limited to total war has not been analyzed in great depth. At the beginning of this century French historians such as Tuetey, Hartmann, and Mention demonstrated that the ferment which accompanied the new, revolutionary warfare began well before 1789. More recent research has shown that the changes most often credited to revolutionaries originated in the last years of the Old Regime.

Marc Martin's work, while not consciously in this stream of thought, provides an important insight into the ideological transition from limited to total war. In his analysis of the military periodicals of the period from 1770 to 1799, Martin concludes that, "born as an instrument for the justification and consolidation of aristocratic privilege in the army, the military press became in thirty years, the means to assure the morale of the soldier" (p. 390). While the latter may be true, a quotation from the *Encyclopédie Militaire* of the Old Regime indicates perhaps some more subtle processes at work. An officer's "place is at his regiment, among his soldiers where he can perfect himself in the art of war; thus the French nobility joining science to the valor that characterizes it so especially will become more redoubtable to the enemies of the state" (p. 36). Even in the Old Regime some military writers considered that more than aristocratic status was necessary, a principle that the Revolution would enshrine. Martin's thoroughly researched and very interesting book hints at these conclusions, but his perception of the break between Old Regime and Revolution is perhaps

overemphasized. Nevertheless, it is a valuable and comprehensive study of a very important subject.

WILLIAM C. BALDWIN  
University of Kentucky

JEAN EGRET. *Necker: Ministre de Louis XVI, 1776-1790*. Paris: Librairie Honoré Champion. 1975. Pp. 478.

This latest book by the foremost authority on the history of France immediately before the Revolution is particularly welcome because traditional interpretations of Necker's career are being rapidly revised. Historians generally have long dismissed Necker as a popular but pettifogging reformer and as a financier whose recklessness did much to ruin the monarchy. Now, however, his distinction as a philosophe has been recognized in the important thesis by Henri Grange, and John Bosher's masterly work on French finances has shown that Necker's despised economies were really fundamental reforms in public administration.

Jean Egret provides a detailed but systematic narrative of Necker's three ministries (1776-81, 1788-89, and 1789-90). Although few perspectives are provided, the reader's familiarity with the period being presumed, the account is clear, concise, and comprehensible. It is also supremely well constructed, each successive step in the story being firmly tied to carefully chosen and fully-annotated quotations from contemporary sources. The author's avowed purpose being to let Necker speak for himself, a major merit of the book is its extraordinarily vivid picture of the minister's situation, aspirations, achievements, and frustrations. This fully confirms the view that Necker consciously sought to end the appallingly wasteful proliferation of private interests in public finance and to establish instead an efficient, centralized administration; Egret is also particularly appreciative of Necker's endeavors to create effective provincial assemblies and to promote a wide variety of humanitarian reforms. We see, too, that in 1789-90 Necker constantly tried to achieve compromise and to restrain the National Assembly from exploiting the assignats.

This is not to say that all is now explained, for the wisdom of Necker's financial (as distinct from his administrative) measures remains open to question, and in places—particularly, perhaps, in the matter of the *Compte rendu* of 1781—the reader may feel that the contemporary debate should be accompanied by fuller scholarly assessment. Egret nevertheless presents Necker as a man who has defects as well as virtues. Ultimately he appears as one who, in impossible circumstances, tried in vain

to persuade the Old Order and the Revolution to listen to the voice of reason.

M. J. SYDENHAM  
Carleton University

ALAN FORREST. *Society and Politics in Revolutionary Bordeaux*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. 300. \$22.50.

Bordeaux at the end of the eighteenth century was a city of elegance, affluence, cultural pretensions, and a real, if unstated, antipathy toward Paris. The opening chapters of Alan Forrest's book provide a fully nuanced portrait of Bordeaux society on the eve of the revolutionary crisis and during the first revolutionary year of cautious optimism. He argues that Bordeaux's politics must be understood initially within a local context, reflecting the interests of the dominant elite of lawyers and administrators and of merchants engaged in overseas trade. The *menu peuple* of Bordeaux apparently lacked either the turbulence or the political consciousness of their confreres in Paris and Lyon. To a surprising degree, they remained throughout the Revolution loyal supporters of middle-class political leadership.

Having described Bordeaux's social and political structure, Forrest shifts to what is essentially a fairly conventional political history of the city from 1791 to 1794. Although Forrest relies almost exclusively on the national, departmental, and municipal archives, the narrative is not significantly different from that found in the works of Bécamps, Lhéritier, and Brace. It is a rather old-fashioned history, perhaps because Bordeaux was a very old-fashioned town.

Forrest argues persuasively that the central revolutionary event was the Federalist revolt of 1793. With the nearly unanimous support of the sectional assemblies, the city convened a *Commission Populaire de Salut Public*; it somewhat vaguely declared that it sought to "regain" Bordeaux's "rights." Like the Gironde's deputies to the National Convention (now imprisoned or in hiding), the Commission was far better at bold pronouncements than it was at effective action. With the defeat of the Federalist revolt, the "commercial and legal middle classes of Bordeaux" lost the political power they had held unchallenged since the beginning of the Revolution. The Terror, however, changed nothing permanently. After 1815, Bordeaux was essentially the same bastion of social conservatism and political moderation that it had been in 1793.

Forrest's conclusions are disappointingly inconclusive. One would have liked fuller comparisons of Bordeaux's revolt with the rebellions of

other provincial capitals of the south, as well as some attention to the social and political structure of postrevolutionary Bordeaux as a demonstration of the author's contention that essentially nothing had changed. Instead, he simply repeats points made earlier concerning the localism and the class-oriented character of Bordeaux's Revolutionary politics.

CLARKE GARRETT  
Dickinson College

MARTYN LYONS. *France under the Directory*. New York: Cambridge University Press. 1975. Pp. x, 259.

General surveys of the Directory have usually treated the period as a betrayal of the Revolution when licentiousness and personal enrichment replaced equality and self-abnegation. Furthermore, these histories have often explained this regime and the motivations of its opponents by a crude social and economic determinism. Martyn Lyons has tried to rectify this; in administering the cure, he has opened some new wounds.

New monographs and articles, assiduously synthesized by the author, reveal that France from 1795 to 1799 was not terribly unlike France before the Terror. The politics, the educational policy, styles and taste, and attitudes toward religion seem to be logical extensions of earlier positions. Even the economic decisions of the Directory followed the general course outlined by the National Assembly from 1789 to 1791 and did not set new standards for greed. To be certain, the wealthy prospered from the sale of sequestered property, but they had done so under earlier governments. Likewise the poor suffered, but they had always been without the basic necessities. The wars and the extraordinarily frigid winter of 1795 caused many more hardships than the programs of the government. The Directors then carried on the policies of their more moderate predecessors. Although Lyons may underestimate the idealism of the early revolutionaries, he seems to be correct when he argues that the Revolution continued after 1794.

But why did the Directors pursue (or preside over) such a policy and why did they face so much hostile opposition? The author does not appear to confront these questions directly, although he clearly tries to deal with them. He carefully examines the immediate concerns, the political machinations, and the actions of the Directors and their antagonists, yet he proffers no systematic, even if tentative, explanation for the motivations of the government and its detractors. In short, captivated by the crosswinds and the vagaries of politics, he never gives a coherent interpretation of the intentions and goals of each side.

In part this failure has compensations, for many of the past historians of the Directory, who have tried to find underlying causes, have often been simplistic. They have suggested that the political struggle of 1795 to 1799 was in large part a projection of a wider economic conflict. Lyons, on the other hand, argues that political alliances were formed around a variety of social, geographic, economic, and particularistic concerns. For example, he argues that the Jacobin opposition of the Year VI was composed of the Parisian sans-culottes and portions of the provincial middle classes. Implying that members of the middle classes adhered to Jacobinism for a variety of reasons, he asserts that in Toulouse their support was based on fear of the promonarchist peasantry who greatly outnumbered them.

Although Lyons skillfully avoids an exclusively economic interpretation of Directorial politics, he does not seem to be able to settle on any general explanation for the behavior of various factions. At one juncture the Directors represent the perspective of the Plain against the middle classes and the sans-culottes; later they stand for themselves and the bureaucracy against the legislative bodies and the nation; finally they work to further middle-class interests. There were probably some continuities in the policies of the Directors; if there were none, there should be some general reasons for this lack of constancy. The author does not detect any general trends in the views of other political and social groups.

Lyons has placed the Directory in the revolutionary tradition, but he has not explained convincingly why the Directors acted as they did and why they were so bitterly opposed. Nonetheless, his contribution is significant, for his book will shift the attention of historians to the latter half of the French Revolution. And he has opened the door to a new, more complex, interpretation of the relationship between politics and class under the Directory.

JACK R. CENSER  
*College of Charleston*

*Les relations franco-belges de 1830 à 1934.* (Acte du Colloque de Metz, 15-16 novembre 1974. Centre de Recherches Relations Internationales de l'Université de Metz, number 7.) Metz: Centre de Recherches Relations Internationales de l'Université de Metz. 1975. Pp. vi, 367. 72 fr.

This collection of essays contains valuable contributions on French-Belgian diplomatic and economic relations. Without presenting new hypotheses or startling revisionist concepts, the twenty-four articles nevertheless produce worthwhile new documentation and insight on the fundamental

issues for these two neighboring states for over a century.

Two constant factors appear to dominate the relations. For France, the security problem prior to 1870 meant absorbing all or part of Belgium, and thereafter meant making Belgium as much a zone of French influence as possible, primarily through economic and/or military agreements. For Belgium, the perpetual problem was safeguarding its political and economic independence from France, Germany, and then Great Britain, the *troisième larron* in that delicate process.

Seven articles produce outstanding results in delineating the reasons for conflict and bitterness and the modes of cooperation in the political, diplomatic, and economic fields. J. Stengers superbly documents the serious menace to Belgium at the birth of the Second Empire, particularly the meaning of the separatist forces in Belgium, the extent of French interest in annexation or invasion, and the precise nature of the British guarantor stance. P. Miquel, J. Willequet, and G. Trausch examine the problems posed by German war aims during and after the Great War, with emphasis on the Belgian desires at the peace conference, the new French-Belgian economic ties after the war, and the critical status of Luxemburg. The French design to bring Belgium out of its neutrality and into the French security plan, and Belgium's fight to avoid satellite status are both comprehensively documented. To me, the three pieces on commercial and industrial relations between 1850 and 1914 (K. Veraghtert, F. Roth, and M. Gillet) are of particular interest, for they depict in an exact fashion the growth of Franco-German antagonisms and the "in-between" position of Belgium. It is clear that, whether viewed through the maritime rivalry of Antwerp and certain French ports, the Belgian industrial interests in Lorraine, or the Belgo-German coal and steel competition with France, patterns of industrialization and the lines of interstate trade severely strained the Paris-Brussels connection before the war. They also convey additional evidence on the meaning and odious consequences of Germany's emergence as an economic titan.

One hopes that Raymond Poidevin of Metz will in the future organize productive and sorely needed explorations similar to those in this volume.

PIERRE-HENRI LAURENT  
*Tufts University*

BERNARD REARDON. *Liberalism and Tradition: Aspects of Catholic Thought in Nineteenth-Century France*. New York: Cambridge University Press. 1975. Pp. viii, 308. \$23.50.



Although Robert R. Palmer's *Catholics and Unbelievers in Eighteenth-Century France* was first published in 1939, its reputation endures. In part this is a tribute to its sound scholarship; in part it is evidence that losers in an intellectual debate do not often attract investigators. Although Palmer was scrupulously fair with his subjects, his study confirmed that Catholicism had been weakened in the war of ideas with the thinkers of the Enlightenment. The same observation could stand for the nineteenth century. Although in the aftermath of Napoleon there was a clear move away from the intellectual positions of the previous century, Catholicism benefited only temporarily and peripherally from the new romantic mood. In fact, as Bernard Plongeron has demonstrated, many of the promising movements of accommodation that he has termed the Catholic Enlightenment were submerged in the trauma of the Revolution and in their association with the Constitutional Church. While the new emphasis on history presented a climate favorable to the examination of the human past for some principle of continuity or for some traditional moral justification for the human adventure, the rapid transformation of the European world during the nineteenth century profoundly complicated the task of those who sought a firmer philosophical defense for Christianity. Since the rapidity of the social and political evolution was matched by a succession of new intellectual fashions from the ideologues to the antipositivists, the burden of the religious thinker was heavy indeed.

Bernard Reardon of the University of Newcastle upon Tyne in this volume assays the effort of those in France who undertook the philosophical defense of Christianity. Some of the authors evaluated are old standbys, such as de Maistre, de Bonald, Lamennais, Maine de Biran, and Loisy. But it is the merit of this volume that it rescues from relative oblivion the writings of Louis Bautain, August Gratry, and Léon Ollé-Laprune. There are short biographical sketches of twenty-seven thinkers, summaries of their views, and critical estimates of their contribution. The footnotes reveal acquaintance with standard secondary works, and they buttress the text with apposite quotations in French on key issues.

Some important conclusions emerge. The intellectual debate of the nineteenth century was not a simple confrontation of those who would return to an idealized past with those who sought new blueprints for an advancing humanity. Among those studied here are some who protested against an exaggerated emphasis on reason and science; but—especially among the lesser known—there were others who anticipated the existential trends of the twentieth century. In this respect, Maurice Blondel deserves the attention given him here and

in recent scholarship. Further, the conflict was not merely between those who had abandoned religious faith and those who had retained it, but it was an equally vigorous encounter among disparate interpretations of the religious heritage, especially between the defenders of unswerving loyalty to tradition and proponents of accommodation.

Reardon has made a contribution to the intellectual history of the nineteenth century by analyzing the writing of a group of thinkers who are generally known only by name. Since these losers represented a strain of thought that still persists in France, this volume deserves a place beside Palmer's.

JOSEPH N. MOODY  
Boston College

ROGER PRICE, editor. *Revolution and Reaction: 1848 and the Second French Republic*. New York: Barnes and Noble. 1976. Pp. 333. \$21.50.

The nearly simultaneous publication of two collections of essays, *Revolution and Reaction: 1848 and the Second French Republic* and *1830 in France* (1975) is impressive testimony to the flourishing state of American and English scholarship on the first two French revolutions of the nineteenth century. *Revolution and Reaction* is composed of a long introduction and eleven essays. The introduction, written by the editor, Roger Price, is an examination of "the roots of the Revolution of 1848 in the social and political structure of the July Monarchy" and of "the process by which the republic became conservative." It is a reasonably accurate and useful synthesis of recent writing on the subject. Two of the essays, those by Bernard Moss and Peter Amann, have been superseded by the authors' books, and the substance of a third, by Charles Tilly and Lynn Lees, was published in *Annales* in 1974.

All of the remaining eight essays offer some new insight into the Revolution of 1848 and the Second Republic, and several give tantalizing tastes of the authors' larger projects. Patricia O'Brien, Jonathan House, and Howard Machin are concerned with institutions of repression—police, armed forces, prefects. John Merriman offers a case study of repression in three localities. Christopher Johnson seeks, in structural changes in the clothing industry, the origins of the Parisian tailors' commitment to cooperation, and Robert Bezucha examines republicans' use of folkloresque events to give public expression to forbidden political ideas.

For me the most original and enlightening of the essays are those of Ted Margadant and Vincent Wright. Margadant presents a fascinating analysis of the insurgency in the Drôme in December 1850 that will require a revision of the accepted view of

the republican resistance in the south. Most readers will, I expect, join me in hoping for the publication of his dissertation, "The Insurrection of 1851 in Southern France." Wright recalls what recent historians, influenced by revisionist judgments of Napoleon III, have ignored—the widespread suffering and humiliation caused by the repression of December 1851–March 1852, not by the formal punishments alone but by the losses of fortunes, businesses, offices, jobs, status, and respect by thousands never convicted of any wrongdoing. The essay makes comprehensible the republicans' bitterness in succeeding decades.

DAVID H. PINKNEY  
University of Washington

ROBERT A. NYE. *The Origins of Crowd Psychology: Gustave LeBon and the Crisis of Mass Democracy in the Third Republic*. (Sage Studies in 20th Century History, number 2.) Beverly Hills: Sage. 1975. Pp. 225. \$13.50.

PETER H. AMANN. *Revolution and Mass Democracy: The Paris Club Movement in 1848*. Princeton: Princeton University Press. 1975. Pp. xviii, 370. \$14.50.

Both of these books consider a basic issue of modern historical scholarship, namely, how people behave in groups and in turn influence policies and actions. Robert A. Nye shows how one social scientist, Gustave LeBon, resolved the nature of mass behavior. Peter H. Amann goes directly to the clubs of the French Revolution of 1848 to see not only what happened, but also why and how men reacted in a particular revolutionary situation. Both authors concern themselves with social psychology; Nye is interested in it as it relates to the history of ideas, and Amann as it explains specific historical events. Both books are of interest to the social historian, but they must be looked at separately, since their purposes and eras of investigation differ.

Nye reviews the writings of LeBon on crowd behavior. A nineteenth-century student of modern psychology and sociology, he like all pioneers can be taken to task by those who follow; yet someone had to begin crowd studies. Nye recaptures the French intellectual milieu of LeBon's age, with its heritage of positivism and its conditioning to prevalent scientism. Not recognized by the academics, a second-rater among contemporary thinkers (Weber, Durkheim, Sorel, Bergson, H. Poincaré), LeBon sought by bombast and prolific popular writings to wield influence. He appealed to lay social scientists over the heads of academe, and his interpretation of collective behavior reached the impressionable. Nye senses the impact of the Commune upon LeBon, who emphasized the crowd "mind," its potential destructiveness, its primitive

instincts, its organic model, its hallucinations, its servility, its imperiousness. What seems to frighten Nye is how easily an adept and popular social analyst with inadequate data and scientific jargon influenced policy, particularly Third Republic leaders and pre-World War I military circles, and by implication modern fascists. Realizing how misconceptions in natural science can create havoc, is it not all the more dangerous if misconceptions in the behavioral sciences, in this instance social and crowd psychology, are promoted among a receptive and gullible lay audience? But, like Nye, we must honor LeBon for his valiant and innovative early effort to bring the crowd under the microscope of social science. This book is an important addition to the history of social thought.

Peter Amann, on the other hand, approaches his study of a mass movement in democracy not by emphasizing theory, but rather by focusing on the phenomenon itself. LeBon drew his conclusions from contemporary evidence and called it science; Amann returns to the historical record. Thus he follows the modern practitioners of crowd studies—Rudé, Soboul, the Tillys. He gathers his data and draws his conclusions from the available evidence; it is for later scholars, using studies such as this, to propose the hypotheses about crowd psychology. Amann moves with caution. In a readable narrative, the author carefully sifts the available evidence. A true historian, he is forced to fill in gaps intelligently. The February revolution spawned the popular clubs, which, Amann conservatively estimates, enrolled at their height nearly seventy thousand. What difference did these nearly two hundred societies make in the format from February through the June Days? Amann notes where the club influence was minimal and where it was crucial. Relying on thorough research in French archives, following such earlier studies as McKay on the National Workshops, forsaking the retelling of the revolutionary story, he provides the solid analysis upon which to build new interpretations. The clubs, with their goals of indoctrination, modest confrontation, and election campaigning, now have their successes and failures exposed. Amann grapples with the ultimate problem of why the Parisian club movement failed, and, like the master of the material that he is, he poses the question of what this tells us about people and why and how they function as they do.

JOHN J. BAUGHMAN  
De Pauw University

ANDRÉ KASPI. *La France et le concours américain, février 1917–novembre 1918*. In three volumes. Lille: Service de reproduction des thèses, Université de Lille. 1975. Pp. 1395.

This substantial study begins by asking a simple question: How important was American aid to France in the First World War? In the course of answering it, André Kaspi illuminates a series of American, French, and international themes. As its size suggests, this is a richly detailed work, drawn very largely from a mass of archival material on both sides of the Atlantic. The American papers (those of House, Lansing, Wilson, Pershing, etc.) have often been exploited, but the French documents are new, having been locked up for almost fifty years and made available only in the last decade. A tremendous volume of papers from the French High Commission in the United States, from the army, and from the Ministry of Foreign Affairs is displayed here for the first time. What is missing, and always will be, is any French equivalent of British cabinet minutes. Nonetheless, ministerial attitudes and decisions can be perceived otherwise. This work was some ten years in the making, and as is usual with the *doctorat d'état* dissertation, there is such a lavish display of evidence, such spacious argumentation, and such generous quotation that even the most assiduous amateur of the subject could scarcely wish for more.

The finding is that United States participation was crucial for France at four levels: financial, economic, psychological, and physical. This will not be news to Americans who have always suspected it; it may be more nearly so to Frenchmen who have not. The interest for all, however, is in the hard evidence exposed and weighed to calculate the precise American contribution from the winter of 1917 to the armistice. Kaspi warns against the pitfalls of statistics, but his work swarms with them—on money, fuel, armaments, munitions, airplanes, shipping, railways, food, and troops. Shipping was the key to nearly everything, determining how, in what proportions, and when United States aid would reach France. Unprecedented international cooperation in this matter was necessary to assure that the indispensable, the most desirable, or the mutually agreed combinations of food, men, and armaments reached France. Here Etienne Clémentel, André Tardieu, and Jean Monnet loom large, and it is clear that Kaspi sees the beginning of that "European" thought which led to the creation of post-1945 West European institutions associated with the name of Monnet (*"l'inspirateur,"* in de Gaulle's sarcastic epithet).

The famous quarrel over "amalgamation" of United States expeditionary forces is examined in great detail. Even Pétain gave up in the face of Pershing's intransigence, but the enraged Clemenceau refused to do so. Kaspi disentangles

fairly the immediate concerns, ultimate intentions, and unexamined prejudices that blinded Americans and French to one another's position, showing how Americans blamed the French for problems no one could solve and how the French unjustly criticized Americans for difficulties inherent in the war situation. He insists on the revolution in French thinking, from early indifference to United States manpower and concern primarily for delivery of materiel, to the most pressing calls for transport of infantry and machine guns before all else; from an early naïveté about America's interests to the realization that the United States had very concrete goals (about which Tardieu had had few illusions from the outset). The work is replete with details of this critical refusal of the Americans to be and count for less than their slowly developing war potential permitted, and of the changing French views of this growing independence of power and ambition. Excellent use is made of the French *commissions de contrôle postal* materials, which provide extracts from intercepted and copied French mail. The purpose then was to gauge public opinion among the civilian population and those in uniform and to assist in mounting effective propaganda. The result of the analysis here is a fascinating study of shifting attitudes and changing stereotypes of the American.

Behind the *histoire événementielle* of the Franco-American collisions over war priorities and United States replacements, of the great hope the Expeditionary Corps gave rise to and the off-duty activities of the doughboys, and of American mismanagement of aircraft production and mishandling of the Renault tank manufacture, looms a more ominous theme of the rising economic might and imperial design of the United States, apprehensively discerned by both the French and the British. Nineteen-nineteen would have seen the Americans militarily paramount after the extraordinary build-up through the previous summer and fall. Their original combat readiness in 1918 was made possible by huge French stocks of artillery, munitions, tanks, and aircraft; by 1919 United States production would have made them more nearly self-sufficient. Thus when the armistice intervened, the political struggle was in the open. In Kaspi's opinion, the United States was strong enough to try to impose its view that the "Americanisation" of political and economic life coincided with what was in the best interests of all. He acknowledges that both France and Great Britain had also fought the war with their eyes very much focused on the postwar settlement. But he seems to believe that fundamental responsibility for failure to obtain general European reconciliation after November rested with the Americans, whose pres-

ence had suddenly become nearly overwhelming. "America's mission was indeed a crusade," he says, "but on behalf of America's interests."

This conclusion is perhaps open to question and qualification, but the richness of the documentation and the interest of the narrative and analysis of this study are quite clear. Kaspi's work is a fine example of that peculiarly French research marathon, the *doctorat d'état*, that J.-B. Duroselle (Kaspi's supervisor) celebrated and justified again some few years ago. It is an intensely absorbing consideration of a Franco-American encounter that was part of the interplay between the New World and the Old in this century.

JOHN C. CAIRNS  
University of Toronto

FRANK FIELD. *Three French Writers and the Great War: Studies in the Rise of Communism and Fascism*. Cambridge University Press. 1975. Pp. 212. \$13.95.

Concerned with the impact of the World War I on political liberalism, Frank Field has turned to three writers who fought in the war and who were subsequently attracted to fascism (Drieu and Bernanos) or communism (Barbusse). This approach has its drawbacks since statistically most French soldiers came away from the war as they had gone in: liberals—at least as voters in the 1920s and 30s. Still, if the jilting of the Third Republic and the embracing of Vichy by millions of Frenchmen in 1940 is any indication, this in-depth exploration may reveal more of substance than some quantitative approaches.

To ascribe the politics of the three writers primarily to the war, however, is dubious. If all three were members of the same church, "the universal church of soldiers living and dead," they were hardly of the same denomination. Despite Field's attempt to find points of commonality between the three—we are told all were romantics who looked back to the comradeship of the trenches as a source of inspiration—it is their political differences, usually rooted in prewar experiences, that prevailed. From Field we learn that Barbusse subscribed to a vague kind of socialism and pacifism even before the war and that Bernanos was the product of a devoutly Catholic and royalist family. The intellectual and emotional foundations of Drieu's politics were also formed well before 1914 (by influences and relationships largely neglected by Field). Nor does the war seem to have been decisive in the later twists and turns of their political thought. Barbusse went from libertarian socialism to Leninism and Stalinism less because the trenches had made him an authoritarian than because the democratic Left seemed incapable of

seriously resisting either war or fascism, two of the most powerful authoritarianisms of the era. Bernanos, a previous admirer of Drumont and Maurras, supported Franco at the outset of the Spanish Civil War but turned against him and against the French and Spanish Right in general when they betrayed the principles of Christian charity that Bernanos imagined to be at the heart of true fascism. His critique stemmed not from the war but from the daydreams of sainthood of a Catholic childhood. Field, therefore, seems to deny much of his own evidence in taking at face value his subjects' view that the war was "the central experience of their lives."

Yet Field transcends his own thesis in providing us with a complex interweaving of motivations and events that affected three lives. Unfortunately in doing so, he resurrects a number of canards about Drieu, including Drieu's alleged commitment to socialism (Field relies too heavily on the work of Drieu's old friend, Pierre Andreu), but such shortcomings are outweighed by the many insights Field does sustain and by an excellent writing style.

ROBERT SOUCY  
Oberlin College

DAVID WINGEATE PIKE. *Les français et la guerre d'Espagne, 1936-1939*. Preface by PIERRE RENOUVIN. (Publications de la Sorbonne "NS Recherches," number 7.) Paris: Presses Universitaires de France. 1975. Pp. 467.

This book is doubly misleading. It is not, as its title suggests, a study of the French and the Spanish Civil War but rather of the French press, which, the author asserts, constitutes our only guide to public opinion in a period prior to the appearance of polls. Furthermore, it is scarcely more than a simple translation of the author's Ph.D. thesis, little revised or updated since its appearance in photo-offset in 1968. Even within these limitations David Pike's study is disappointing. There is no analysis of the structure of the French information media, of the readership of the various newspapers in question, of circulation beyond simple numerical estimates, or of ownership—this last having been a burning issue for supporters of the Popular Front. No attempt has been made to assess the real political influence of the press either, and the author has refrained from trying to evaluate the quality of reportage. One knows that the fascist press regularly lied, and that the Communist press gave news from a Marxist perspective (also lies for Pike), but one would like to know to what extent the more responsible newspapers, if there were any, provided their readers with more or less accurate descriptions of a very complex reality. Only



the numerous charges and countercharges of gun-running by Right and Left and alleged transgressions of the Franco-Spanish frontier have been somewhat illuminated by the author's access to the records of French Prefectures bordering on Spain. For the rest, Pike has provided a careful and thorough, but often tedious and undigested account of the editorial reaction of most of France's major newspapers, including key provincial ones, to the more significant diplomatic and military milestones of the Spanish conflict.

Little that is surprising emerges from this recounting although a good deal is of interest. Pike shows, for example, that most of the right-wing press allowed even such incidents as Guernica to pass in silence or printed nationalist claims that the carnage was the work of Anarchists acting from within the city. The socialist and trade-union press (particularly *Le Peuple*) tended to share the Communist view of the Barcelona street fighting of May 1937, labeling the Anarchists as "fascist auxiliaries." Pike does a good job of demonstrating the evolution of French conservative journalists like Pertinax and Henri de Kerillis away from a pro-Franco posture, particularly after right-wing sentiment divided under the impact of the Munich agreements.

Where Pike ventures beyond the press his judgments must be considered as suspect, especially since he fails to acknowledge or to incorporate in his work any of the results of recent scholarship. The only secondary sources relied upon are dated if solid studies by Puzzo and Micaud; as important works as those of Colton and Lefranc do not appear in the footnotes or in the index. There is no bibliography of secondary sources, although one is promised by the author on request (efforts to secure it by this reviewer reveal, however, that it is not yet ready and will not be until some time in 1977 at the earliest). Thus Pike labels nonintervention a British proposition, supposedly forced on Léon Blum by a Baldwin threat that Britain would not be bound by Locarno in the event of French complications in Spain, and by the Radicals, who threatened to withdraw from the Popular Front coalition. But recent studies based on the now open Public Records Office and French diplomatic documents would rather appear to have established that the policy originated in the *Quai d'Orsay*, and that it was highly popular even in the ranks of Blum's own Socialist party. Pike's concluding judgment is that Blum should have hermetically sealed the Franco-Spanish frontier in August 1936 in a unilateral gesture of good faith, and that French leakage across the border of arms and material for the Loyalists in some way "justified" intervention on behalf of Franco by the fascist powers that otherwise might not have taken

place. It should suffice in this context to say that this thesis cannot be substantiated on the basis of the impressionistic newspaper evidence that makes up the bulk of the volume; those interested in a more substantive and eloquent refutation have only to consult Pierre Renouvin's preface to Pike's work.

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MARK POSTER. *Existential Marxism in Postwar France: From Sartre to Althusser*. Princeton: Princeton University Press. 1976. Pp. xii, 415. \$17.50.

In his impressive study of *Existential Marxism in Postwar France* Mark Poster has grappled with two of the most complex movements in recent intellectual history. On the one hand, he has traced the development of French Marxist theory from the discovery of Hegel and the "humanist" Marx in the 1940s, through the struggles between the new Marxism and the Stalinism of the French Communist party in the 1950s, to the formation of a New Left in the 1960s. At the same time he has described the evolution of existentialism from the relatively apolitical doctrines of Jean-Paul Sartre's *Being and Nothingness* to the radical existentialism of the 1960s. Poster's thesis is that a great deal of the most significant thought in postwar France may be viewed as an attempt to form a synthesis between existentialism and post-Stalinist Marxism and that a merger between the two was achieved during the 1968 student revolts.

Most earlier studies of existential Marxism have been restricted primarily to the works of Sartre (Wilfred Desan's *Marxism of Jean-Paul Sartre*, Walter Odajnyk's *Marxism and Existentialism*, James Sheridan's *Sartre, The Radical Conversion*, and R. D. Laing's and Duff Cooper's *Reason and Violence*) or to the works of Sartre, Albert Camus, and Maurice Merleau-Ponty (Germaine Bree's *Camus and Sartre*, Michel-Antoine Burnier's *Choice of Action*, or Raymond Aron's *Marxism and the Existentialists*). But Poster has surveyed the entire milieu in which existential Marxism developed and has analyzed not only the works of Sartre, Camus, and Merleau-Ponty, but also those of Lucien Goldmann, Henri Lefebvre, Claude Lévi-Strauss, Louis Althusser, the *Arguments* group, and a number of writers who are less well known. In view of the enormous volume of demanding writings produced by these thinkers, Poster's skillful description of their interactions is a major scholarly achievement.

Unlike such students of contemporary French intellectual history as H. Stuart Hughes, Poster has remained sensitive to the philosophical nuances of his subject matter. But at times his



sympathy for the existential Marxists has led him to accept their claims somewhat uncritically. For example, he provides no criticism of the historical validity of the discovery of a "humanist" Marx. And his descriptions of the May revolts of 1968 are romanticized.

Moreover, Poster's desire to bring this vast body of writings within a single explanatory system sometimes causes him to oversimplify. He has performed an important service in demonstrating the ideological origins of the French New Left, but he has established so much continuity throughout this period that at times the ideas of the 1940s and 1950s seem to be teleologically drawn toward the apocalypse of 1968. Despite these weaknesses, Poster's work provides an invaluable guide to the development of leftist thought in postwar France, and *Existential Marxism in Postwar France* is a fine example of the traditional methods of the history of ideas brought to bear on a contemporary topic.

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GUSTAV UNGERER. *A Spaniard in Elizabethan England: The Correspondence of Antonio Pérez's Exile*. Volume 1. (Colección Támesis. Series A: Monografías, number 27.) London: Támesis Books. 1975. Pp. xxxix, 505. £19.50.

Antonio Pérez was a Spanish courtier who fled his native country following his participation in a conspiracy against Juan de Escobedo, secretary to Philip II's brother Don Juan. Eventually he arrived in England (1593-95), where he became a protégé of the Earl of Essex and sought revenge on his former king by writing polemical works against Spain. His name appears in a number of standard books on Elizabethan history, and a biography of him has been published by Gregorio Marañón. He has remained, however, a very shadowy character, sometimes confused with the Portuguese pretender Don Antonio who was also an exile in England.

In the first of two projected volumes, Gustav Ungerer has assembled all the known letters to and from Pérez between 1590, when he was still in France, and 1595, when he returned to France on a mission of Essex. It is a prodigious work, well supplied with footnotes, and with letters in Spanish, Latin, English, and French. Some are already published in various collections, but many appear here for the first time.

The editor indicates various uses to which the collection might be put, including a contribution to a standard biography of Essex. But if we ask whether Pérez's importance justifies the labor which has gone into this collection, the preliminary answer must be no. He was at best a third-

level figure, and his involvement in the late humanist movement, his services to Essex, and the possibility that he was the model for Don Adriano de Armado in *Love's Labours Lost* do not seem to require the kind of scholarly treatment not yet given to a number of his more important contemporaries.

The organization of the book also limits its usefulness. "The arrangement of the material is a compromise between chronology, subject-matter, language, and correspondents. The order of presentation corresponds to the chapters of a future monograph on Pérez's residence in England," according to the editor. The result is somewhat confusing, with letters grouped first with others written in the same language and then according to time period, making it necessary to refer continually to the general chronological table at the beginning of the book in order to pursue any phase of Pérez's career closely. The many letters which appear in sixteenth-century Spanish or French and are untranslated further restrict the work to a relatively few specialists. A smaller selection of documents, arranged in precise chronology and translated, would have been more serviceable to the general student of Elizabethan history.

Unfortunately, the editor has interspersed certain parts of the book—both in a sectional introduction and in footnotes—with his own dubious and amateurish psychoanalytic speculations. ("Hence it is not illogical to assign to Essex the function of a father figure and to interpret the second visit [of Pérez] as the struggle to revert to parental protection.")

JAMES HITCHCOCK  
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VICENTE CÁRCCEL ORTÍ. *Política eclesial de los gobiernos liberales españoles, (1830-1840)*. (Instituto de Historia de la Iglesia, Universidad de Navarra.) Pamplona, Spain: Ediciones Universidad de Navarra. 1975. Pp. 530.

FERNANDO DE MEER LECHA-MARZO. *La Cuestión religiosa en las cortes constituyentes de la II república española*. (Instituto de Historia de la Iglesia, Universidad de Navarra.) Pamplona, Spain: Ediciones Universidad de Navarra. 1975. Pp. 212.

"How is it possible?" wrote Francesco Tiberi, the nuncio to Madrid, to the papal secretary of state in 1828, "that after serving the Holy See for thirty-three years without hearing a single reproach from anyone, as soon as I cross the Pyrenees, I become transformed into some sort of imbecile?" A century later, Nuncio Federico Tedeschini could have written the same lines about his feud with Cardinal Pedro Segura. Spaniards, clergy and laity alike, have seldom been fond of Rome's envoys.

These two volumes, covering turbulent periods of church-state relations a century apart, make this point clear. Fernando de Meer Lecha-Marzo's work is a brief study of the course of the 1931 Spanish Republic's legislation against the Church. Aside from the recently published letters of Cardinal Vidal y Barraquer, it contains nothing substantially new in either documentation or interpretation.

Vincente Cárcel Orti's work is another matter. Exhaustively documented, it covers the decade from the birth of Isabella II in 1830 through the beginnings of the Carlist controversy to the end of Maria Cristina's regency and the end of the First Carlist War. During this period, anticlerical governments legislated the end of most clerical privileges and initiated the *desamortización* of church properties. A tradition of violent anticlericalism began: churches were burned and nearly two hundred clerics were killed. It was the first wholesale liberal attack on the Spanish Church, and the clergy responded with condemnation and renewed their support for the Carlist cause (which, after all, was one of the chief reasons the clergy were attacked in the first place).

Cárcel Orti depicts much of the action through the eyes of the nuncios. Despite the title, the book deals more closely with Spanish-Roman relations than with the intricacies of the Spanish governments' anticlerical policies, and the reports of the Madrid nunciature are thoroughly integrated into the text. When Tiberi arrived in Madrid, a number of regalistic problems were already outstanding—most notably those concerning patronage rights over the newly independent American sees. Then, when Ferdinand VII died in 1833, Gregory XVI refused *de jure* recognition to Isabella, and the Regent's government responded by refusing to recognize Tiberi's successor, Luigi Amat, who nonetheless came to Madrid to report to Rome on the situation. After Mendizábal's radical anticlerical government began the *desamortización*, Rome broke off diplomatic relations with Madrid in 1836 and did not resume them until 1845.

A study of both church and state in chaos emerges from Cárcel Orti's work. The liberals' attack upon the encrustation of clerical privilege—the result of centuries of intertwined church-state relations—resulted in nothing less than the destruction of the old Church's structure, and the post-1840 attempt at an impossible restoration still had its reverberations in Spanish politics a century later, in the attitudes of 1931.

Cárcel Orti concludes with a survey of both the state of the clergy and the people in that doleful decade, along with a list of all sixty dioceses and their bishops (of whom only eleven remained in

charge of their sees in 1840). His book is a valuable contribution to the history of a turbulent century.

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JOSÉ ANDRÉS GALLEGO. *La política religiosa en España, 1889-1913*. Madrid: Editora Nacional. 1975. Pp. 519.

Religion in the politics and life of modern Spain has played a surprisingly small role in recent historiography. Non-Spanish historians, in particular, are attracted to the intensity and bitterness of sectarian politics during the Civil War. As a result, other areas of Spanish studies have lagged: it has only been a few years since monographs on Spanish liberalism, the dominant force during the nineteenth century and Catholicism's principal enemy, have been available.

Spanish historians, on the other hand, have paid much more attention to religion as an ingredient of national life. José Manuel Cuenca, for example, published his provocative *Estudios sobre la iglesia española del XIX* in 1973, and now José Andrés Gallego has written this volume under review on the politics of Catholicism. Both draw upon the work of the late Melchor Fernández Almagro, certainly the best of the Spanish historians on the second half of the nineteenth century.

The chief problem that Catholicism faced during this period was an adamant refusal of Antonio Cánovas del Castillo and Mateo Sagasta, the main architects of the restoration, to allow any force—be it military, religious, or regional—to dominate the liberal government. They did this, unfortunately, only with the strong backing of southern *latifundistas* and other economic oligarchs. For the northern ultramontanes, the situation was doubly frustrating because the Cánovas-Sagasta regime remained too conservative to attack in the manner of the Carlists in 1833 or 1873.

Gallego treats this stalemate as a kind of quiet Kulturkampf that went on into the early twentieth century, occasionally rising from the level of subdued skirmishing into national politics. The lobbying and campaigning by a variety of Catholic spokesmen, including Pidal, Cascajares y Azara, and Polavieja, or of groups and parties such as *Unión Católica*, the Integralist party, *El Partido Católico*, and *Acción Católica*, are all given excellent coverage.

By 1898, however, after the "disaster" of the Spanish-American War, when the restoration system lay in ruins, Spanish Catholics were still disorganized. The corporativism of *Rerum Novarum* was unappreciated and unavailable as a means of counterattacking the new onslaught of anticlericalism thrown up by radicals like Lerroux or Blasco

Ibáñez. The defeat in Cuba raised demands for reform of such magnitude that religion found itself threatened with a withdrawal of public funding. Some of the best chapters of the book deal with the political controversy of the period 1898-1912 and the broad conversion of Catholic voters into conservatives deeply interested in maintaining the status quo by whatever means possible. No other work has managed to do this as successfully, and without partisanship or parochial tone.

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JOAQUÍN ROMERO MAURA. *La rosa de fuego—republicanos y anarquistas: La política de los obreros barceloneses entre el desastre colonial y la semana trágica, 1899-1909*. (Colección "Historia Contemporánea.") Barcelona: Ediciones Grijalbo, 1975. Pp. 649.

The historian who like Joaquín Romero Maura decides to navigate such well-charted waters as the decade 1900 to 1910 usually chooses one of two routes. One is to sail the familiar passage, providing new data for some established seamarks and for the relocation or redefinition of others. An alternative is to strike out on another tack, to provide knowledge from a different perspective such as that of the new social history or a quantitative approach. While contemporary political and sociological models are used for comparison, Romero Maura has chosen basically the traditional course of political history. Determinedly revisionist, he seeks to relocate almost every seamark.

To this task he brings rich gleanings from archives abroad and in Spain; the latter are invaluable, for he alone has had access to many of the Spanish archives that he cites, at least to date. His Oxonian training and extraordinary literary skill have enabled him to make full use of the documentation. Curiously, this appears primarily in the footnotes, as a supplement to the text which is based on a detailed analysis of Alejandro Lerroux's newspapers, *La Publicidad* and *El Progreso*. Moreover, he has added to the already extensive bibliography of printed documents, books, and statistics. His revisionism, however, rests more on new interpretation than on the new data per se.

Romero Maura's forte is analysis of the decision-making process in the Constitutional Monarchy, specifically of the way in which the "regenerationist" impulse that erupted into public life after Spain's disastrous defeat in 1898 was channeled into the existing political system rather than into a revolution. One group, the Catalán regionalists, eventually swelled the mainstream of Conservative party politics, but the major impact was the revitalization of the nondynastic left (that is,

the Republican movement). This book focuses on the most innovative republican group, reorganized and led by Alejandro Lerroux in Barcelona, and on the conditions of the workers whose vote he sought. Rejecting descriptions of Lerroux as a demagogue as well as the persistent socialist and anarchist charge that *lerrouxismo* constituted "mixtificación y desviación," Romero portrays the movement as a sincere, skillfully mobilized worker party that articulated the aspirations of most Barcelona workers, be they militants or voters. *Lerrouxista* ideology, he argues, relied less on anticlericalism or on patriotism than on offering a *political* solution to workers' problems. Even the week of convent burning and barricades of July 1909 constituted a political action, designed to remove a real obstacle (the Church) to the modernization of Spain.

Romero is firm about the validity of his interpretations and the fallibility of alternatives, which he regards as "an error" at best, "a *canard*" at worst (refreshing clarity in our relativist world). But while Romero Maura's study of the dynamics of republican politics in this era constitutes a major scholarly achievement, his treatment of the republicans' rivals, the anarchists, is so thoroughly revisionist as to incite controversy. He is at his best in an analysis of the anarchist militant (although the portrait is hardly flattering). But his treatment of anarcho-collectivism and syndicalism presents a "view from above": of governors like Ossorio, or police files, or even of a middle-class republican like Lerroux who sought workers' votes. His findings and certainly his viewpoint will be subjected to intense scrutiny by the many historians who have focused precisely on this period in Spanish history.

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*Mare Luso-Indicum: Études et documents sur l'histoire de l'Océan Indien et des pays riverains à l'époque de la domination portugaise*. Volume 1. (Centre de Recherches d'Histoire et de Philologie de la IV<sup>e</sup> Section de l'École pratique des Hautes Études. Fourth Series. Hautes études islamiques et orientales d'histoire comparée, number 2.) Geneva: Librairie Droz, 1971. Pp. xiv, 168, 3 maps.

This is the first in a projected five-volume series of studies and documents on the history of the coastal areas of the western Indian Ocean during the period of Portuguese hegemony, beginning in the early years of the sixteenth century. The series is under the general editorship of the eminent French historian of Iran, Jean Aubin, who, if this volume is typical, will also be its principal author.

While scholars may differ on the extent to which the Portuguese penetration of the Indian Ocean was a turning point for world history, all would agree that it marked an important stage in the expansion of Europe and the eclipse of the eastern Mediterranean and Middle Eastern areas. There is not, however, a single comprehensive and reliable history of the first two centuries of Portuguese activity in the Indian Ocean. Aubin's series is intended to fill, at least partially, this lacuna by bringing together copious evidence from Portuguese sources with that from Oriental sources (Indian, Arabic, Persian, and Turkish).

The narrative portion of this volume consists of three studies. The first is Jean Aubin's on Alfonso de Albuquerque's activities on the coast of Gujerat from approximately 1510 to 1515; Geneviève Bouchon writes about the kings of Kōttē (the major principality of Ceylon) in the sixteenth century; and, finally, Jean Aubin describes Albuquerque's attempts in 1507 and 1508 to impose Portuguese dominance on the Persian Gulf entrepôt of Ormuz. The emphasis in all three essays is on problems of chronology and the political strategies of both the Portuguese and the indigenous political rulers of the Persian Gulf and the principalities of the western coast of India. In all three, facts (many of them newly discovered) and analysis are blended with great skill.

This volume makes abundantly clear that the technological superiority of the Portuguese in naval warfare was not the major factor to which scholars may attribute their success; the weakness of the indigenous polities contributed at least as much. When confronted with an insightful and able adversary like Cojeator in Ormuz, the task of the Portuguese viceroys was not an easy one.

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ANTÔNIO DE FIGUEIREDO. *Portugal: Fifty Years of Dictatorship*. New York: Holmes and Meier. 1976. Pp. vi, 261. \$13.50.

This book deals with the full career of the late dictator Antônio de Oliveira Salazar, discussing his personal background and ideology, his financial policies, the elaboration of the Estado Novo, and the new African empire. Some important pages concern the Spanish Civil War, the Second World War, the deflationary economy of the fifties and sixties, and the collapse brought about largely by the regime's anachronistic colonial policies. Portuguese imperialism, Antônio de Figueiredo writes, was an extension of feudalism, or, in another passage, a capitalism without capital. The Colonial Act of 1933 actually increased the gap between whites and Africans. The notion of "as-

similation," proudly claimed as evidence that the Portuguese were not racist in their policies, included so many parochially European traits that no African could qualify for the status without completely rejecting his own cultural heritage. At home the regime, as measured by censorship, conformist gestures, secret police power, and numbers of political prisoners, became more oppressive after 1945 than it had been during the 1930s and World War II. In economic matters also Salazar looked principally backward, creating and then unsuccessfully struggling to hold on to, a purely mercantile empire in which Africa supplied raw materials and was not permitted to manufacture anything that Portuguese industry could claim to be able to produce. The writing is impressionistic rather than comprehensive, based on reliable sources but not nearly as copious in specific information as is Hugh Kay's *Salazar and Modern Portugal*. Its principal value is the empathy and personal experience that enable the author to help the reader *feel* the sorrows and the hopes of ordinary Portuguese people.

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P. GEYL. *Pennestrijd over staat en historie: Opstellen over de Vaderlandse Geschiedenis aangevuld met Geyl's Levensverhaal (tot 1945)*. With a foreword by J. C. BOOGMAN. (Historische Studies, number 27.) Groningen: Wolters-Noordhoff NV. 1971. Pp. 386. 38.50 gls.

This volume comprises reprints of four earlier studies by the late Pieter Geyl: "The Stadtholder-ship in the Party Literature under De Witt," "Democratic Tendencies in 1672," "The Witten-War: A Pamphlet Controversy in 1757," and "1813 Recalled in 1863." They are supplemented by recollections of his combative journalistic, academic, and political career to 1945, originally recorded on tapes by an interviewer in August and November 1964 and now published verbatim for the first time. Those who can read Dutch will be glad to have the first four papers reprinted within the covers of one book. At first sight somewhat disparate, they form a logical sequence, since they are episodes in what Geyl termed the history of the idolizing of the House of Orange and of the struggle between the religious and the secular strains in the Dutch body politic. They are mainly based on an acute analysis of the pamphleteering and polemical literature of the periods concerned, which Geyl knew and handled so well. Readers familiar with Geyl's work, whether in Dutch or in English translations, are well aware of which side his sympathies lay on; these four articles are typical of his debunking of



the extreme pro-Orangist pretensions and of his admiration for the brothers De Witt. His embattled recollections in 1964, barely two years before his death, stress his passionate involvement in the Greater Netherlands movement and his controversies with some of his academic colleagues. This section is best read in the light of E. H. Kossmann's perceptive comment in the *Bijdragen en Mededelingen betreffende de Geschiedenis der Nederlanden* (88 [1973]: 123).

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STEVEN KOBLIK, editor. *Sweden's Development from Poverty to Affluence, 1750-1970*. Translated by JOANNE JOHNSON. Minneapolis: University of Minnesota Press. 1975. Pp. 380. Cloth \$16.50, paper \$4.95.

FRANKLIN D. SCOTT. *Scandinavia*. (The American Foreign Policy Library.) Revised edition. Cambridge: Harvard University Press. 1975. Pp. x, 330. \$15.00.

These two works cover much of the same ground, yet differ in approach and perspective. Steven Koblik, an American, presents thirteen essays by contemporary Swedish scholars—nine historians, an economic historian (economic history is a distinct discipline in Scandinavia), a sociologist, and two political scientists—on selected social, economic, and political developments since 1750, competently translated from the Swedish edition of 1973. Koblik seeks to provide “dependable and up-to-date information about the growth of modern Sweden,” and the book does this within its areas of concern; notably absent are cultural and intellectual themes, which in Scandinavia are generally left to literary or art historians.

These essays comprise neither an introductory nor a narrative survey. Each is more or less self-contained, and a thread of continuity is provided by Koblik's judicious introduction and commentaries. An unstated aim is to provide material for comparative studies. Certain essays are thus more evidently intended for comparativists, in specialized fields like industrialization, political ideology, or social welfare policy, than for students of Scandinavia as such. The book finally seeks to “acquaint the reader with Swedish historiography and historians.” It gives a good, representative sampling, although unaccountably no personal data on its contributors. Koblik rightly points to the strong Swedish tradition of limited, problem-oriented, monographic scholarship, yet even on the basis of his own collection one may question his stressing the lack of attention to the modern period, of “analytical studies” based on “modern

theoretical foundations,” and of “broad consensus” regarding the period. Surely these are “weaknesses” only as perceived by Swedish historians themselves, and as such they reflect their very real concern with precisely these areas. Non-Scandinavian scholars will thus be impressed with recent Swedish concentration on the past two hundred years, model building and quantification of the quantifiable under strong influence from the social sciences, and the high degree of consensus, as evidenced by a notable lack of bold theses and heated debate. Modern Swedish (and Scandinavian) academic historical scholarship tends to be methodologically fastidious—and rather antiseptically impersonal. An American reader may miss in it the sweat and turmoil of human striving, the “scent of human flesh,” in Marc Bloch's words.

By comparison, the human and personal element is all the more evident in the work of Koblik's former mentor, Franklin D. Scott, which deals with all the Nordic countries and represents an Anglo-Saxon, humanist tradition. One senses throughout the interaction of the individual scholar with his material as he freely speculates, scans broad horizons, and draws upon his rich personal experiences in Scandinavia, often with touches of quiet humor. He provides plentiful, well-selected hard data, effectively utilizing the voluminous statistical information available, yet he never neglects to relate generalizations to concrete human circumstances in their cultural setting. Although Scott is a leading historian in his field, *Scandinavia* is not basically a history but an analytical survey, in historic context, of state and society in present-day Scandinavia, emphasizing social democracy, the economic base, and the search for security in a divided world. It is a thorough revision and updating of his *United States and Scandinavia* (1950). The new title more accurately reflects content and emphasis; yet an implied purpose remains to compare conditions and developments in the Nordic lands with those elsewhere, particularly the United States. The section on “Scandinavian-American Crosscurrents” perhaps recalls most strongly the book's original thrust. Then as now, Scott sought to derive practical lessons, of wider applicability, from Scandinavia's creative confrontation with the common problems of modernity. Many others have attempted the same since Marquis Childs in 1936, but none with better background or judgment. Scott is clearly sympathetic toward the new Scandinavia, yet ever aware of the costs of change and of uniquely favorable circumstances. He credits the Scandinavians with no superhuman wisdom or virtue and sees their social idealism as always counterbalanced by cool pragmatism and enlightened self-interest.

Both books are valuable scholarly contributions



to a field all too frequently dominated outside Scandinavia by special pleaders with a priori views regarding the desirability or practicability of socioeconomic reforms. Koblik provides English-speaking scholars with a welcome introduction to such eminent colleagues as Sten Carlsson, Jörgen Weibull, and Kurt Samuelsson, and with a view of the development of their present society from their Swedish and largely social-science perspective. Scott's work meanwhile demonstrates impressively the critical insight a perceptive humanist outsider can bring to the study of other societies and cultures.

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ROLF TORSTENDAHL. *Teknologins nytta: Motiveringar för det svenska tekniska utbildningsväsendets framväxt framförda av riksdagsmän och utbildningsadministratörer, 1810-1870* [The Value of Technology: Reasons in Favor of Technological Education Given by Swedish Parliamentarians and Administrators, 1810-1870]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, 66.) Summary in English. Uppsala: Almqvist and Wiksell. 1975. Pp. 274.

Rolf Torstendahl's volume, another valuable contribution to the distinguished Acta Universitatis Upsaliensis, is a thoroughly documented and seemingly definitive study of Swedish technological education in the age before heavy industry. He presents contemporary French, German, and English developments and experiments in technological education for comparison and indications of borrowings. Torstendahl clearly demonstrates the gradual shift from largely functional technology to genuinely scientific research, and the growth of conscious social concern regarding the nature and direction of technology.

Following a survey of northern European technological education in the eighteenth and nineteenth centuries, the author turns to the establishment of specific institutions devoted to instruction in mining, engineering, military science, agriculture, metallurgy, and arts and crafts (*slöjd*) at both secondary schools and technological institutes. He devotes much space to the debates of parliamentary leaders and heads of institutes regarding the virtues of practical and theoretical instruction. He describes the simultaneous establishment of state, local, and private institutions, from the establishment of the School of Mines at Falun in 1822 to that of the Technological Institute (1827) in Stockholm, the Agricultural Institutes at Degeberg (1834), Ultuna (1848), and Alnarp (1862), the privately started Chalmers Institute of

Technology at Gothenberg (1829), the Technical Elementary Schools from 1853, and the proliferation of the Swedish *slöjd* societies from the 1840s. Torstendahl then proceeds to a careful analysis of the debates regarding the social benefits of technical education as a factor in the nature of the curricula and the purposes of such education, as well as its relationship to the conventional classical educational programs. The amount and quality of discussions and progress in Swedish technology before modern heavy industry became dominant is impressive. This may help explain Swedish pre-eminence in many technological fields.

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L. A. PUNTILA. *The Political History of Finland, 1809-1966*. Translated by DAVID MILLER. London: William Heinemann; distributed by Crane, Russak, and Company, New York. 1975. Pp. 248. \$12.75.

Neither original nor stimulating, this concise book focuses on Finnish political history in the twentieth century. L. A. Puntila's handling of historical events is competent, but he aims at a low level of sophistication, for an audience that may not know that Harry S. Truman and John F. Kennedy were presidents of the United States (pp. 186, 226). He offers an index of persons, but no bibliography or footnote references.

The most serious errors appear on only a handful of pages dealing with the Finnish Civil War of 1918 (pp. 105-08), the Social Democratic Opposition in 1939-45 (pp. 202-03), and the aspirations of Finnish Communists in 1948 (p. 209), but Puntila, a retired professor, could have avoided them had he made an effort to read and digest the literature about these events that has appeared in the last decade.

More important, the major topics he covers have already received adequate treatment elsewhere. Potential readers of Puntila's book should first consider as alternatives a wide variety of works that have been written in English, or have been translated into English, by James Barros (on the Åland Islands); Douglas Clark and Max Jakobson (on the Winter War); John H. Hodgson (on the Finnish Civil War and Finnish communism); Carl Erik Knoellinger (on the Finnish labor movement); Hans Peter Krosby, C. Leonard Lundin, and Anthony F. Upton (on Finland in the Second World War); Jaakko Nousiainen and Pertti Pesonen (on Finnish political parties); Juhani Paasivirta (on Finland's relations with Great Britain, France, and the United States in 1918-19); Marvin Rintala (on the Finnish Civil War and the extreme

right-wing in Finnish politics); John P. Vloyantes (on Finnish neutrality); and John H. Wuorinen (on nineteenth-century Finnish history).

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KARI SELÉN. *Genevestä Tukholmaan: Suomen Turvallisuuspolitiikan painopisteen siirtyminen Kansainliitosta pohjoismaiseen yhteistyöhön, 1931–1936* [From Geneva to Stockholm: The Re-orientation of Finnish Security Policies from the League of Nations to Nordic Cooperation, 1931–1936]. (Historiallisia Tutkimuksia, number 94.) Summary in English. Helsinki: Suomen Historiallinen Seura. 1974. Pp. 289.

Finland's role in international affairs has increasingly interested scholars in recent years. Kari Selén's study is concerned with the changes taking place in Finland's foreign policy in the early and middle 1930s and complements Keijo Kerhonen's work on the period. During these years Finland, as so many other small states, concluded that it was unrealistic to base its security on the League of Nations. Instead it was going to try to establish closer cooperation with the Scandinavian countries and primarily Sweden. The Finnish foreign-policy decision-makers regarded cooperation with Sweden as the only really viable alternative open for Finland. There was fear and dislike of the Soviet Union and unwillingness to get involved and entangled with Germany, Poland, or the smaller Baltic states. Sweden had a respectable armament industry, and Finns saw it as a possible and the safest source of arms and help for their own armament industry in times of danger. Cooperation with Sweden and the other Scandinavian countries would not endanger Finland's neutrality as cooperation with other countries might.

The pursuit of a rapprochement with Sweden in turn necessitated changes in the internal politics of Finland. Those who favored closer ties with Sweden, including among others the influential military figure C. G. Mannerheim and the Social Democrats, thought it necessary to tone down the long-lasting and bitter feud between the Finnish-speaking and Swedish-speaking elements in Finland. This struggle, which involved the whole nature of the bilingual Finnish republic and its institutions, had awakened strong animosities in Sweden toward ethnolinguistic Finnish nationalism. Eventually the support of the majority of the Finnish parliament was gained in pursuing a rapprochement with Sweden. Yet Sweden remained cautious and unwilling to enter the kind of specific commitments the Finns desired.

Selén writes clearly and matter-of-factly, but he moves too much on the surface of things, busily

citing one diplomatic communication after another. There is no real analysis of the defense issues involved. The writer sees his work as a study in security policy, and it is obvious that the defense and military needs of Finland triggered the pursuit of the rapprochement with Sweden camouflaged under the vaguer and more general term "Nordic Orientation." Yet Selén does not even attempt to analyze the military capacity and the armament industry of either Finland or Sweden or the needs of the former and the ability of the latter to meet them. Beyond a mere tentative statement, he makes no effort to discuss the impact of the ethnolinguistic feud in Finland upon Sweden's attitudes toward its neighbor. A lack of such analysis and discussion is disappointing and disturbing in a book of this nature and leaves it a much narrower and less substantial work than it need have been. There are also some obvious gaps in the documentation and bibliography, although overall the documentation used seems to be carefully cited.

PEKKA KALEVI HAMALAINEN  
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JOHN P. VLOYANTES. *Silk Glove Hegemony: Finnish-Soviet Relations, 1944–1974: A Case Study of the Theory of the Soft Sphere of Influence*. Kent: Kent State University Press. 1975. Pp. xiii, 208. \$10.00.

John P. Vloyantes, a political scientist, asserts that his book on post-World War II Finnish-Soviet relations is a case study of the soft sphere-of-influence theory. "The main utility of history here," he advises us, "is as a source of raw material from which to fashion theoretical constructs." After some thirty pages of constructs, followed by another hundred and fifty pages of raw material, he concludes that there is indeed a "positive correlation . . . between the Finnish-Soviet relationship and the essentials of the soft sphere model." Perhaps so, but historians might wonder about the point of the exercise. The theoretical framework seems designed with the Finnish-Soviet relationship in mind, with passing references to Franklin D. Roosevelt's Good Neighbor Policy thrown in, leaving serious doubts about the general validity of this case study.

Historians will still find the "raw material" of some interest, although specialists have nothing to learn here. Unfamiliar with either of Finland's two languages, Vloyantes draws almost exclusively on published English-language sources, including *Facts on File*, *Keesing's Contemporary Archives*, and the *New York Times*. The result is a superficial if intelligent account of the development of Finland's relationship over the past three decades with its

former great-power enemy. Vloyantes agrees with those who maintain that Soviet policy vis-à-vis Finland is based on requirements of security and defense, and he illustrates with reasonable accuracy how Finland has learned to accommodate its own policy accordingly. Until a thorough study comes along, this book will provide a convenient summary of postwar Finnish-Soviet relations.

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GERHARD BENECKE. *Society and Politics in Germany, 1500-1750*. (Studies in Social History.) Buffalo, N.Y.: University of Toronto Press. 1974. Pp. xi, 436. \$17.50.

The study of early modern Germany has long stood under the shadow of its sequel: the rise of Brandenburg-Prussia as a great power, the Austro-Prussian contest for supremacy in north-central Europe, and the history of Germany from the Battle of Sedan in 1870 to the Battle of Berlin in 1945. Understandably, historians have combed and sifted the period from the Reformation to the eve of the French Revolution for the antecedents and origins of the modern nation-states that have shaped the world in which we live. Because of this preoccupation, however, the history of early modern state and society throughout much of Germany has been largely neglected, particularly in Anglo-American historiography, notwithstanding exceptional contributions such as F. L. Carsten's *Princes and Parliaments in Germany*. This makes all the more welcome the illuminating study by the German-born, British-trained Kent historian Gerhard Benecke, *Society and Politics in Germany, 1500-1750*.

Not taking Berlin or Vienna as his point of departure, but rather Detmold and Lemgo, the residence and the largest city, respectively, of the Westphalian county of Lippe, Benecke deals with his topic at three levels—local, regional, and imperial. The core of his book is a densely documented monograph on the administrative, legal, social, and economic structure of Lippe, a northwest German principality with a perimeter of perhaps thirty miles and a population that probably did not exceed 50,000 by the beginning of the eighteenth century. He couples his study of Lippe with a detailed survey of nearby territorial states in northwestern Germany and their interaction with each other and—individually as well as collectively—with the Holy Roman Empire. Finally, he addresses himself, first in an extended introductory essay and then in his conclusion, to his fundamental thesis: the early modern empire can-

not be adequately understood as a moribund relic of the Middle Ages or as a mere seedbed within which the modern nation-states of the region were to grow inexorably to sovereignty and power; neither the postmedieval nor the premodern view takes into account that the early modern empire was in fact a viable constitutional entity, perhaps baffling in its complexity to latter-day observers, but appropriate and effective for the intricately structured federation of states composing it.

The findings of a number of important German studies that have appeared since Otto Brunner's pioneering *Land und Herrschaft* a generation ago have been explicitly assimilated by Benecke in his extensive and helpfully annotated explanation of the historical context and significance of his own research on Lippe. Consequently his book should prove doubly valuable. For the specialist, it provides a model study of a representative minor principality in the early modern empire. For others, especially those without access to the German literature, the work can also serve as an authoritative introduction to the history and historiography of an age too often written off as a period of transition, yet—as Benecke shows—well worth studying in its own right.

DONALD S. DETWILER  
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WALTER ELLIGER. *Thomas Müntzer: Leben und Werk*. Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. viii, 842. DM 98.

WALTER ELLIGER. *Aussenseiter der Reformation. Thomas Müntzer: Ein Knecht Gottes*. Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 123. DM 10.80.

Although Thomas Müntzer long remained in ill repute among both Protestants and Catholics, historians have given his revolutionary activities favorable treatment whenever there were widespread attempts to make radical changes in society and government. He has received the most attention since the Second World War, when Marxists made him the outstanding hero of the Reformation.

The response of Reformation scholars to attempts to prove that Müntzer was a dialectical materialist engaged in a social and political revolution had resulted in numerous valuable studies, but not in a detailed account of his various activities as related to his inner religious development. It is this gap that Walter Elliger has filled by examining in meticulous detail every shred of evidence concerning this "Servant of God," as Müntzer called himself.

Concentrating on Müntzer's development as a reformer, Elliger points to his overwhelming deter-

mination, from his student days at Leipzig and Frankfurt an der Oder to his execution at Mühlhausen, to ascertain God's will with respect to re-establishing the Apostolic Church. He shows how Müntzer, taking his departure from Luther's doctrine of the Word of God, emphasized the role of the Holy Spirit in speaking directly to the elect, thereby making known God's plan to return to earth in the immediate future to eliminate the "tyranny of the godless" and establish the church of the godly. He documents Müntzer's conception of his own mission as that of leading the Reformation out of the apathy into which he believed it had fallen. He also treats in detail Müntzer's constructive achievements in both theology and liturgy.

As Elliger shows, when Luther branded Müntzer a revolutionary agitator and the princes refused to follow his preaching, the reformer recognized God as his sole ruler whom he would follow by carrying out his apocalyptic vision through his "covenant of the elect," whether these followers were nobles, townsmen, or peasants. The author points out that Müntzer, driven from one place to another because of his activities as an agitator, eventually became associated with the Peasants' Revolt and, at the Battle of Frankenhausen, even served as its leader. When his violent attempt to punish the godless, tyrannous princes collapsed, he did not question God's judgment, but he placed the blame for the revolt's failure on the peasants for not having had sufficient faith in the Holy Spirit and for following their own materialistic interests.

For the purpose of presenting his significant findings to a large number of readers, Elliger has published them in summary fashion in the paperback, *Aussenseiter der Reformation*.

HAROLD J. GRIMM  
Ohio State University

MARK U. EDWARDS, JR. *Luther and the False Brethren*. Stanford: Stanford University Press. 1975. Pp. viii, 242. \$10.00.

From 1522 until his death, Luther had to contend not only with Roman Catholic opponents but also with other reformers whose understanding of the Gospel differed from his. Concentrating principally on Luther's disputes with Karlstadt, Müntzer, Zwingli, Bucer, Agricola, and Schwenkfeld, and dealing with issues as well as events, Mark U. Edwards attempts "to develop a coherent explanation for Luther's claims about himself, for his often brutal attacks on his evangelical opponents, and for the reluctance of those opponents to respond in kind." The author notes that, with the exception of Müntzer and Schwenkfeld, these opponents accepted the central principles of the Reformation; therefore, Luther found it necessary to

separate himself from the errors and misdeeds of his opponents, lest people become confused and the Gospel again be corrupted. According to Edwards, in the early conflicts with Karlstadt and Müntzer, Luther developed a stereotype of a false prophet that he applied to other opponents. All were accused of having a satanic spirit and therefore were capable of misdeeds they neither committed nor advocated. Luther believed that since Adam's fall there had been a constant struggle between Christ and Satan, between the true Church and the false, between true prophets and false prophets. Although at first Luther claimed no special role for himself, gradually he came to see himself in that of the biblical prophets and the apostles.

In contrast, the opposing evangelical reformers generally honored Luther and recognized his contributions to the recovery of the Gospel, but they contended that he also was subject to error and correction. Edwards suggests that the reasons for this restraint lie in the tactical necessity of getting Luther's followers to listen, in the influence of humanism, and in their agreement with Luther on major points.

Edwards' well-written essay, based on primary and secondary material, is adequately annotated. Most of the notes are at the end of the volume, but some appear as footnotes in the text. This arrangement and the lack of a separate bibliography create some inconvenience for scholarly use but do not otherwise impair the quality of the work.

Although Edwards generally seems sympathetic to Luther, he recognizes the intemperate and often unjustified brutality of the reformer's attacks on his evangelical opponents and helps us to see that Luther must bear his share of responsibility for the bitterness that developed within a divided Protestantism.

FRANK J. WRAY  
Berea College

STEVEN E. OZMENT. *The Reformation in the Cities: The Appeal of Protestantism to Sixteenth-Century Germany and Switzerland*. New Haven: Yale University Press. 1975. Pp. xi, 237. \$12.50.

For a number of years scholars have been intrigued by the striking early success of the Protestant Reformation in German-speaking cities, a high percentage of which either quickly adopted or seriously considered adopting this new form of Christianity. Many studies have explored this phenomenon, most of them monographs on particular cities but some of them interpretive essays, the best known of which is Bernd Moeller's *Reichsstadt und Reformation*. Ozment's erudite and ingenious essay

is an impressive new attempt to explain this connection.

After a shrewd and judicious introductory survey of current trends in Reformation research, Ozment begins his argument by analyzing lay religious attitudes on the eve of the Reformation. He finds laymen in general deeply interested in religion but oppressed by the increasingly burdensome demands it places on their daily lives. In the succeeding chapter, which takes up nearly half of the book, Ozment analyzes the original Protestant message as revealed by about fifty popular pamphlets published between 1519 and 1526. He finds that they offer laymen a great release from an overly complex and burdensome set of religious obligations. In his final chapter the author analyzes how Protestants won the support of civic governments and supplied institutional support for the new beliefs. He concludes that people were won to Protestantism not by distant political events or refined theological doctrines but by "the palpable institutional consequences of the Protestant program of reform" in local communities (p. 165).

While I find this conclusion attractive and plausible, I do not find Ozment's demonstration completely convincing. He has attempted to solve a problem in social history with the evidence of an intellectual historian. This evidence, to be sure, is supplemented with copious use of monographs by social historians, but occasional dubious use of these monographs leaves me uneasy. We are left with a very suggestive hypothesis, still in need of full empirical demonstration based on more direct use of the sources from specific cities.

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ROBERT BIRELEY. *Maximilian von Bayern, Adam Contzen S.J. und die Gegenreformation in Deutschland, 1624-1635*. (Schriftenreihe der historischen Kommission bei der bayerischen Akademie der Wissenschaften, number 13.) Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 241. DM 46.

This carefully-researched and detailed work is a case-study of two age-old questions. What role did "religious concerns" play in the Thirty Years' War? What influence did the Jesuits have upon the formulation of policy by the secular princes to whom they were sent? The example is Adam Contzen's relationship to Maximilian of Bavaria, whom he served as confessor during the critical years 1624-1635.

Robert Bireley is more successful in answering his first question than his second. Contzen and Maximilian were ideally suited to one another.

Indeed, Contzen's early political writings, in which he characterized the Catholic faith as the best guarantee of civic good and emphasized the duties of the prince to that faith, seem first to have brought him to the deeply pious Maximilian's attention. It was entirely natural, then, that Contzen should advise this prince on a variety of matters, although his superiors in fact warned him not to associate himself—or the Society of Jesus—too closely with the politics of someone who remained a German prince. For Maximilian and his confessor, the Thirty Years' War was fundamentally a religious struggle.

The author's conclusion that Maximilian's "Catholic expansionism" derived from Contzen (and therefore from at least one Jesuit) does not, however, necessarily follow. For example, Bireley grants that a hard line on the Calvinists was consistent with Maximilian's self-interests in the Palatinate as well as his religious convictions. The author nonetheless holds firm to his argument with the remark that Maximilian's claim about the Calvinists' forming a disruptive party "was not without a certain historical justification." In sum, the author's conclusions are insufficiently nuanced, in spite of the almost overwhelming detail with which they are presented.

Contzen and Maximilian nevertheless remain a powerful counter-example to generalizations about the politics of the period based upon Richelieu and Louis XIII.

JAMES M. KITTELSON  
Ohio State University

NOTKER HAMMERSTEIN. *Jus und Historie: Ein Beitrag zur Geschichte des historischen Denkens an deutschen Universitäten im späten 17. und im 18. Jahrhundert*. Göttingen: Vandenhoeck & Ruprecht. 1972. Pp. 405. DM 75.

A clearer understanding of parliamentary history has often resulted from the emphasis of a given nation's historians on its legal history. While successful in the British case, the elaboration of such an understanding failed in Germany because of the Prussian-centered emphasis of its historians on administrative and institutional history rather than on legal history proper. Even the best modern German historians have lacked a solid foundation in German legal history. Notker Hammerstein's thorough research into the old Empire's jurisprudence and the development of Reich Historie and constitutional law (*Jus publicum*) as combined university subjects after the Peace of Westphalia will therefore benefit studies of the decline of the Holy Roman Empire and of the subsequent problems of state organization in Central Europe.

The new kind of jurisprudence and history first



emerged at the Prussian University of Halle. The Halle school was founded by Christian Thomasius (1655–1728), one of the fathers of the German Enlightenment, who articulated the new approach to history and law as university disciplines. Rooted in the Lutheran tradition, his new humanist orientation nonetheless represented a departure from the theological emphasis of the Reformation. Jurisprudence replaced theology as the main subject, and its study was justified as necessary and useful for maintaining the institutions of the state, in this case the old Empire with its tradition of “German freedom.” There was a focus on resistance to imperial absolutism as well as to reasons of state. For Thomasius religion was a private affair, and all human actions and beliefs were subordinate to a man’s conscience. In order to defend the original freedoms of the German princes, it was necessary to advocate the separation of German and Roman legal traditions. This meant historical research into the meaning of the old German customary laws. Hence modern law, concepts of the modern state, is tied to the development of constitutional law. The teaching of it then became a mark of the modernity of a university.

Thomasius’ influence was carried forward into the German Enlightenment by Johann Peter von Ludewig (1668–1743), and Nicolaus Hieronymus Gundling (1671–1729). Ludewig was an adviser to Frederick the Great and wrote a justification of the Prussian claim to Silesia in 1741, and tended to support a territorial absolutism emerging by the early eighteenth century while at the same time idealizing the freedom of the German princes. He argued that the essential constitution of the Reich had not changed since its founding, but Gundling was more modern than this. His views influenced Gatterer and later Ranke who further developed the German tradition of writing scientific and “truthful” history which was already apparent in the Halle school. Gundling went beyond writing Reich Historie as legal history and already emphasized that it was not merely the laws which were in process of development, but the people, their institutions, and their spirit. Herder and Hegel are adumbrated. The state is a secular phenomenon which reflects “the will of God,” and jurisprudence was the discipline which dealt with maintaining and promoting this order in the world.

With the rise of Göttingen University in the early eighteenth century, the Halle school was transplanted to the West. Baron G. A. von Münchhausen, the Hanoverian minister who directly developed the faculty and curriculum of that university, was a follower of Thomasius. The emphasis on history continued. Gundling directly influenced J. J. Schmauss, and Johann Christoph

Gatterer, who influenced Ranke directly, developed a more modern kind of historiography. Gatterer expanded the content of history to include more than the compendium suggested by the Halle school. The history of the Reich had also to be the history of the German people and it had to be “unpolitical” (*sic*) and true. Enlightenment historians like Voltaire, Robertson, and Hume were rejected as writers of fiction.

Hammerstein’s work is the first modern study of the historiography of the old Reich in the important period after 1648, and will be indispensable to all future research of the German historical tradition as well as to historians of early modern Germany. It is the most brilliant work appearing on this subject in many a decade and no student of German parliamentary institutions can afford to remain ignorant of these new perspectives.

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HUBERT C. JOHNSON. *Frederick the Great and His Officials*. New Haven: Yale University Press. 1975. Pp. vi, 318. \$17.50.

Hubert C. Johnson’s book is essentially an attempt to describe administrative and bureaucratic developments in the reign of Frederick II from the standpoint of the new governmental imperatives which accompanied Prussia’s meteoric rise to great-power status in the eighteenth century. These imperatives could not be met within the framework of the collegial board system that Frederick’s father established for an earlier, less ambitious state. Johnson sees Frederick as “the radical promoter of a new state” by virtue of his receptivity to new ideas and his willingness to experiment with novel forms and organs of administration to solve the new problems created by war, economic reconstruction, the acquisition of additional territories, and the maintenance of great-power status.

In some respects, the story Johnson tells is not new, and it will not surprise specialists in this area. Some points which emerged in other, earlier works are: Frederick’s dissatisfaction with the structural and attitudinal limitations of the General Directory and other collegial bodies; his decentralization of administration through creation of new and more functional agencies; the close cooperation between Frederick and the relatively few ministers whom he chose (or accepted) as the executors of a significant half-step in the direction of what would ultimately become ministerial government; and the resistance to change of what Johnson calls the “core bureaucracy” of the central and provincial boards. Yet the author adds to this general picture many fascinating glimpses into the

way the administration actually worked—or, in some cases, did not. From his description arises the picture of a Prussian state still quite unmodern in many ways. The imperfectly defined spheres of public and private interests, the continual bickering and backbiting of officials, and the constant necessity of king and ministers to bargain with, cajole, and bribe the socially important corporate groups—perhaps even more often than to threaten and coerce them—combine to make the old picture of a Frederician royal despotism in Prussia, as well as the more recent one of monolithic bureaucratic omnipotence and omnicompetence, virtually as inappropriate for Prussia as for any other eighteenth-century European state.

As a study which seeks to explore the operation of the Prussian administration as well as its organization, Johnson's book brings a fresh and interesting perspective to a critical period of Prussian history. It is a welcome addition to the literature.

JOHN G. GAGLIARDO  
Boston University

FRANZ JOSEF PITTSCH. *Die wirtschaftlichen Beziehungen Bremens zu den Vereinigten Staaten von Amerika bis zur Mitte des 19. Jahrhunderts.* (Veröffentlichungen aus dem Staatsarchiv der Freien Hansestadt Bremen, volume 42.) Bremen: Selbstverlag des Staatsarchivs der Freien Hansestadt Bremen. 1974. Pp. 278.

Franz J. Pittsch's work, based largely on the holdings of the Bremen State Archives and on U.S. consular records, is a perfect example of the classical German dissertation: exhaustive of its subject, painstakingly researched, packed with information, and limited in the scope of its conclusions.

It traces the development of trade relations between the Weser port and the United States from its beginnings, shortly after the end of the American Revolution, to the middle of the nineteenth century. Despite the disparity in the trading partners' size, population, and political significance, this trade was no small matter. In the late 1830s the city imported about a third of the total annual U.S. tobacco crop; it served as an important Central European entrepôt for such other American exports as cotton and whale oil, and it became, with the development of dependable packet service in the 30s, and of steam navigation in the late 40s, the leading gateway for Central European emigration to the United States.

The book is of considerable interest to students of German economic history and commercial policy of the early nineteenth century. It presents impressive concrete evidence as to the effects of industrialization and the consequences of the development of new communications means and routes and of the commercial policy of the *Zollver-*

*rein*. It also casts revealing light on such American topics as the growth of the port of New York at the expense of its rivals, especially Baltimore, and the decline of the American merchant marine from its dominant position in the decade following the Napoleonic wars. By 1839, 81 percent of the ships sailing between the U.S.A. and Bremen carried the latter's flag! The book includes useful statistical tables and appendices.

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University of Vermont

HANS BOLDT. *Deutsche Staatslehre im Vormärz.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, volume 56.) Düsseldorf: Droste Verlag. 1975. Pp. 320. DM 72.

Hans Boldt's subject is the theory of constitutional monarchy in early nineteenth-century Germany, with constitutional monarchy defined as a system of dualism dependent on cooperation between monarch and popular representatives in joint decision making. The theory focused on the problem of balancing the interests of representatives and monarch, and regulating conflicts between them.

Contemporary theorists, Boldt argues, were practical men, who took their point of departure from the only significant political system they knew; existing governments did in fact function on a basis of compromise between ruler and assembly. Constitutional theorists valued this system in its own right, as combining the virtues of order and stability through the monarch, popular liberty through representation. Their aim, then, was not to create theoretical constructs, but to help the existing system function effectively; if the essence of the system was that neither monarch nor assembly had the power of final decision, then the problem had to be stated in such terms. Boldt sees many variations, reflecting the different political allegiances of individual writers, within the general dualistic theory. What he rejects is the tendency he finds among constitutional historians to adopt Stahl's argument, which posited a dichotomy between the two principles of monarchical and parliamentary government and implied that one or the other principle must be dominant in any state. Boldt is persuaded that even writers like Rotteck and Mohl, usually seen as exponents of parliamentary government, did not accept the reality or desirability of such mutually exclusive systems. That the system of constitutional monarchy ultimately proved impossible because the question of sovereignty could not indefinitely be evaded does not affect the case; for contemporaries it set the terms of debate.

Focusing sharply on a limited range of questions permits Boldt to deal fully with complexities, and

it makes possible a more sympathetic treatment of thinkers such as Rotteck and Zachariä, who are often dismissed as superficial and schematic. Throughout Boldt makes explicit his differences with various contemporary scholars, pre-eminently Huber. The heart of the study, and its chief value, consists of an astute and lucid analysis, often of absorbing interest, of theory in relation to practical problems.

LENORE O'BOYLE  
Cleveland State University

DIETER LANGEWIESCHE. *Liberalismus und Demokratie in Württemberg zwischen Revolution und Reichsgründung*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 52.) Düsseldorf: Droste Verlag. 1974. Pp. 494. DM 72.

Dieter Langewiesche's book is an example of the recent German emphasis on regional history. While comprehensive in its treatment of its specific topic, it could be better related to German history in general as well as to other regional experiences. The author has clearly devoted long hours to research in archival and newspaper sources; his coverage of the secondary literature is thorough. Although it will probably be read cover-to-cover only by specialists, it constitutes an important source for our knowledge of the 1848-70 epoch in Württemberg.

The conclusion of the study is unexciting—the split between liberals and democrats rendered the middle class impotent and incapable of demolishing the authoritarian state system. According to Langewiesche, the hostility between these two groups in Württemberg stemmed from both social change and the movement for unification. Liberals feared the final results of lower-class requests for complete political equality that democrats supported, while the latter could not accept cooperation with a Prussian-dominated state.

At times the discussion is hard to follow. What is the point of arguing that the liberals demonstrated more continuity during these years? It is true that some democrats finally decided in favor of the Prussian solution to unification, but many liberals changed their minds and accepted universal suffrage. Nor is it clear why the *konstitutionelle Prinzip* and republicanism should be treated as if they were mutually exclusive. The description of the liberal-democratic evolution in Württemberg is well documented and convincing, but one wonders whether there was an alternative, especially after 1866, to this intestine political struggle. Why did not the two groups recapture their "temporary" unity of 1848? The split is described, but not entirely explained.

Finally, in a lengthy work of this type, the organ-

ization should be less mechanical, less given to a multitude of chapters and subsections that detract from rather than add to the unity of the work. The tables in the section on economics are confusing and hard to read, and there is no good reason why an author should refer the reader to another section of his work on practically every page. Still, these are minor weaknesses in a solidly researched study of a significant subject.

JAMES F. HARRIS  
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College Park

WALTER STEITZ. *Die Entstehung der Köln-Mindener Eisenbahngesellschaft: Ein Beitrag zur Frühgeschichte der deutschen Eisenbahnen und des preussischen Aktienwesens*. (Schriften zur Rheinisch-Westfälischen Wirtschaftsgeschichte, number 27.) Cologne: Rheinisch-Westfälischen Wirtschaftsarchiv. 1974. Pp. 387.

The bibliographical heading of Walter Steitz's book implies another exercise in academic futility: a doctoral dissertation published as part of a series by a local press. What, after all, can be said about a subject so frequently discussed that it is virtually common academic property? Can additional research do more than supplement arguments previously advanced? Fortunately Steitz was not so pessimistic. He begins by establishing three interpretations of Prussian railway developments. Conservatives stress the positive role of the state and its bureaucrats. Liberals instead describe a struggle of farsighted entrepreneurs against a system refusing legal or financial support. Marxists present an initial conflict between feudalism and the bourgeoisie that gradually evolved into mutual cooperation against the emerging proletariat. Steitz then proceeds to demonstrate that all three viewpoints have been so concerned with fitting a specific example into an analytical framework that they blurred or distorted the actual events. His careful examination of municipal and state archives and contemporary periodical literature leads him to a set of commonplace, common-sense conclusions. Railway projects were initially developed in the Rhineland to aid local economies, but they rapidly outgrew provincial concerns. The railroads' projected impact was enough to arouse government interest from the first. But it was a cautious interest, particularly since promoters tended to seek government support when opportunities for profit seemed too low to attract private investment. Hostility to railroads came instead from firms or individuals economically threatened by them—a category transcending class lines. But Steitz takes pains to show that minds could change. Railroads were a seminal innovation whose financing, plan-

ning, and building required the development of corresponding new attitudes and techniques. Investors, entrepreneurs, and bureaucrats alike were involved in a complex learning process offering everyone ample opportunity to make and rectify mistakes. Stupidity, malice, and class conflict played less-significant roles in the actual evolution of the Köln-Minden railway than in its discussion by synthesizers who tend too often to combine preconception and hindsight. Steitz shows that it is indeed impressive what historians can find in archives when they take the trouble to look.

DENNIS E. SHOWALTER  
Colorado College

DENNIS E. SHOWALTER. *Railroads and Rifles: Soldiers, Technology, and the Unification of Germany*. Hamden, Conn.: Archon Books. 1975. Pp. 267. \$15.00.

*Railroads and Rifles* is a monograph in military history that does not consider such familiar questions as the relationship of army and society and the social composition and outlook of the officers' corps. Rather, it concentrates on the army as a "military instrument." The book shows how armies—mainly the Prussian—met the "challenges of the industrial revolution" by utilizing technology and adapting strategy and tactics to such developments as railroads, steel, rifling, and breech-loading mechanisms.

Dennis E. Showalter presents Prussian officers and military leaders as properly cautious in accepting innovations, but basically alert to military applications of technology and aware that new weaponry implied altered tactics and retraining of personnel. Moltke's heavy reliance on railroads, the early adoption of the needle gun, and the acceptance of steel-barreled cannons are impressive evidence of their progressive attitude. Nor were Prussian officers slow to learn from observations of armies in action. The war against Schleswig-Holstein proved that masses of troops and supplies could be moved effectively by rail. The Crimean War afforded evidence of the effect of rifle fire on infantry. The value of long-range cannons was manifest in the attack at Düppel.

Somewhat technical, but not without interest, is the discussion of tactical changes made in response to rifles, with their greater range and fire power. The slowness to abandon mass attack formations in the face of greatly strengthened defense suggests an incredible conservatism on the part of European armies. Yet, as Showalter suggests, commanders were understandably reluctant to reduce their control over their soldiers by adopting loose skirmish lines on the attack.

The nonmilitary historian will find a full fare of not so widely known facts that will tempt him to

speculate far more than does the author. For example, the still "secret" needle gun came into the hands of the rebels who stormed the arsenal in Berlin in June 1848. Did this development help to harden the government's position against the revolution? A French general inspected the Prussian Musketry School in 1864 and reported to Napoleon III that Prussian musketry was the best in the world. Did the report have a significant bearing on the nonbelligerency of the French in 1866? The organization and weapons of the armies of the North German states in the 1860s were similar to Prussia's. Was this similarity an element in the political unification of the north?

We must not fault the author too much for not following up these lines of inquiry, for such was not his purpose. What Showalter has given us is a highly readable, well-documented, and well-argued book. At a time when social, economic, or psychological analysis underlies so much historical writing, it is a refreshing change of pace to read a work that simply presents men confronting new technologies and mastering their use on testing ground and battlefield.

ROBERT W. LOUGEE  
University of Connecticut

EKKEHARD BÖHM. *Überseehandel und Flottenbau: Hanseatische Kaufmannschaft und deutsche Seerüstung, 1879-1902*. (Studien zur modernen Geschichte, number 8.) Gütersloh: Bertelsmann Universitätsverlag. 1972. Pp. 418. DM 34.

This welcome monograph, a Fritz Fischer-directed dissertation, traces the relationships among the Hanseatic merchants, classic free traders, and the complex of economic and political interests behind German navalism. The emphasis is on Hamburg with sometimes contrasting consideration of Bremen. These international merchants faced economic threats from agrarians who wanted barriers against American and Russian food imports and from industrialists who wanted to keep out foreign competition in finished goods. Abroad they encountered growing tariff walls, especially in the United States and Latin America, and the possible loss of their foreign investments during revolutions or native insurrections. To this politically sophisticated group the navalism of the late 1890s presented both an opportunity and a danger. An enlarged cruiser fleet would protect their foreign investments; further, it would imply victory over their protectionist opponents. But a large fleet based on battleships would inevitably increase European tensions and disrupt the stability so essential to international trade.

Böhm convincingly demonstrates how Admiral von Tirpitz gulled the Hamburg merchants into



supporting the 1898 and 1900 navy bills. He promised them cruisers and induced them to hope the navy law would mean the eclipse of the agrarians. He then sold them out. He built not cruisers for gunboat diplomacy but battleships that threatened their best customer, England. In the struggle for hegemony among merchants, industrialists, and agrarians, Tirpitz used his influence in favor of a *Sammlung* of the latter two. The merchants paid for the fleet through increased commercial taxes, the industrialists got fat contracts to build it, and the agrarians got grain tariffs as a consolation prize. The merchants' political influence faded after 1900 along with their naval enthusiasm.

Some flaws in this book deserve notice. Böhm asserts that initially Tirpitz really intended a major part of the fleet for direct commerce protection. V. R. Berghahn's *Der Tirpitz Plan* (1971) proves the contrary, but apparently it was published after Böhm completed his manuscript, though it is in the bibliography. The narrative ends in 1902, and this is a disappointment. It would be interesting to know how the Hanseatic merchants reacted to the 1906 navy bill. Its ostensible purpose was to build six large cruisers for service abroad. Some of them were to be built in the Blohm and Voss yards in Hamburg. Did they support that bill, too, or did they finally see through Tirpitz's deceptions? These flaws, however, are minor compared to the value of this well-researched monograph.

PATRICK J. KELLY  
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HELMUT BLEY: *Bebel und die Strategie der Kriegsverhütung 1904–1913: Eine Studie über Bebels Geheimkontakte mit der britischen Regierung und Edition der Dokumente*. Foreword by GUSTAV W. HEINEMANN. Göttingen: Vandenhoeck and Ruprecht. 1975. Pp. 254. DM 34.

Among the might-have-beens of Imperial German history, two issues occur with peculiar frequency: Anglo-German relations and the development of German socialism. In this extended essay, Helmut Bley combines both topics by investigating Bebel's peace initiative toward England. On the basis of eighteen previously unknown conversations and seven letters between the aging leader of the SPD and the British consul in Zurich, Angst, (from September 1910 to August 1913) he tries to clarify Bebel's foreign policy ideas and determine his impact on British decision making.

Because Bebel felt powerless to reform Prussia (despite the growing electoral success of his party) and was convinced "that we find ourselves at the eve of the most horrible war which Europe has ever seen" (p. 143), he took the extraordinary step of repeatedly warning the British government of the aggressive designs of the German navy. He argued

"that there is only one way to end the ruinous naval race between England and Germany, and that is the floating of an overwhelming special loan for the royal navy" (p. 156)! With this "reverse-Holstein" policy of entente through British armament, he tried to achieve the goals of "double hegemony" (England on sea, Germany on land) and "eastward expansion" (p. 135ff.), thereby preventing the coming war. Bley is correct in calling this "a surprising and utterly mistaken solo (*Alleingang*)," since it strengthened the hand of the British naval, Entente, and pro-war party and undercut the hesitating reconciliation efforts of the Bethmann-Hollweg government.

While his strength lies in placing Bebel within the socialist discourse ("his conceptions were not those of a thoroughly international socialist," p. 80), Bley's weakness is his description of the Imperial German government system, which he reduces to currently fashionable negative clichés. Typical is a certain double standard that leads him to quote from the British war party without qualifications, while he severely criticizes any phrase that might be construed as belligerent from the German side. His thesis that Bebel's "decisive mistake in judgement" was the assumption that "the German government system was rational enough to accept without aggression an international situation which strongly limited its possibilities for expansion . . ." (p. 93) prevents Bley from seeing that Bebel's more important failure was his reluctance to choose between a clearly revolutionary Marxist strategy of overthrowing the authoritarian system or an evolutionary social democratic strategy of transforming Imperial Germany from within. Either would have enabled the political power of German socialism to be used more decisively in favor of peace and freedom.

Although important for future biographers of Bebel and interesting because of its two chapters on British attitudes toward Germany, Bley's book has considerable shortcomings as an analysis of the external and internal dynamics of Imperial Germany. At times it appears to be making a mountain out of a molehill—but I must add in fairness that it is a most imposing molehill.

KONRAD H. JARAUSCH  
University of Missouri,  
Columbia

HANS MOMMSEN *et al.*, editors. *Industrielles System und politische Entwicklung in der Weimarer Republik: Verhandlungen des Internationalen Symposiums in Bochum vom 12.–17. Juni 1973*. Düsseldorf: Droste Verlag. 1974. Pp. 1017. DM 87.

This hefty volume of more than a thousand pages presents the papers and discussions from a week-



long international symposium at the University of Bochum (Germany) in June 1973. The subject of the conference—the interaction of economics and politics during the Weimar Republic—is of course crucial for an understanding of the many crises that beset that state; and the list of participants, some seventy of them, including a few Frenchmen, some Britishers, and a number of Americans, is impressive. The subject matter was divided into seven “working sessions,” each devoted to anywhere from six to eleven papers of varying length and quality. The titles of the various sessions give some idea of the wide range of subjects covered: economic growth and changes in the industrial system; social policy and social conflict; financial policy and its effects on social divisions; the international framework and the role of reparations; the influence of economic interest groups—industrial; the influence of economic interest groups—agrarian and *Mittelstand*; and state intervention and interest politics in the [final] crisis.

Given the diverse backgrounds of the participants—historians as well as economists and political scientists—and given their often narrow specialties, it is not surprising that the keynote of the conference was diversity rather than consensus. At each session a reporter (*Berichterstatter*) began by summarizing, with differing success, the arguments presented by the contributors, in the hope of finding some common ground for discussion. But no sooner was the floor opened to debate, when differences among the members soon dissipated whatever semblance of agreement the *Berichterstatter* thought he had discovered. The resulting discussions, while stimulating to the participants, leave the more general reader, looking for some larger themes and conclusions, bewildered and confused rather than enlightened. To be sure, some general trends emerge again and again—the importance of interest groups, especially in heavy industry, for Weimar politics, or the key role of the reparations issue—but these are hardly startling, and even they were not unanimously agreed upon.

In a final session a deliberate effort was made to derive some general lessons from the week-long debates. Again differences of opinion undid what little consensus seemed to emerge. In his concluding remarks the *Berichterstatter* for this final session humorously reported a conversation he had had the night before with the porter of his hotel. “Do you realize how much this conference costs?” the man had asked, adding: “Why spend such a tremendous amount of money for something that is long past and can no longer be changed?” Our reporter had agreed that such expense was probably difficult to justify, and his partner had concluded: “If you, as a historian,

find this difficult to justify, I as a taxpayer find it completely impossible.”

This reviewer does not take quite so dim a view. There were doubtless ways in which this symposium could have been improved, and much of the participants’ self-criticism seems justified. But as an example of fruitful academic debate on a truly international level, the Bochum conference made a great contribution, and the resulting volume will provide valuable information and stimulus to future students, not only of the Weimar Republic, but of modern industrial society in general, if only by having shown the complexities of its subject.

HANS W. GATZKE  
Yale University

MICHAEL H. KATER. *Studentenschaft und Rechtsradikalismus in Deutschland, 1918–1933: Eine sozialgeschichtliche Studie zur Bildungskrise in der Weimarer Republik.* (Historische Perspektiven, 1.) Hamburg: Hoffmann und Campe. 1975. Pp. 361. DM 58.

This work is the first systematic investigation of a subject that has recently aroused much interest: the widespread student support for National Socialism before 1933. Kater’s book, which is not as broad as his title promises, focuses on two areas. The first section includes an analysis of the social background and problems of Weimar student life, while the second explores the National Socialist student movement. His description of the hardships students faced offers a case study of the impact of Weimar economic conditions. The tension between the traditional student image and the reality of hard times tended to magnify the students’ stress at the same time that it limited public awareness of student difficulties. The failure of the Republic’s student policy is amply documented.

Kater is less sure when he touches on the problem of Weimar social structure. His three broad “classes” inadequately describe the period’s social complexity. Kater applies this imprecise approach to stratification to a social analysis of the National Socialist students based upon limited evidence, drawn for the most part from the atypical University of Würzburg. This is a major handicap, since Kater’s conclusions assume that the Nazis attracted a representative group of students.

Kater’s topical approach to the National Socialist students is likely to confuse readers who come unversed to this subject. It permits him, however, to isolate those issues such as *Wehrsport*, anti-Semitism, violence, and compulsory labor service, which were essential elements of Nazi appeal. While he occasionally assumes that student support for National Socialism demonstrates support

for specific aspects of the National Socialist program, his treatment of the ambivalent relationship between Nazi students and the parent party is a suggestive contribution to our understanding of the nationalistic section of the *Bildungsbürgertum* in the Nazi years.

Scholars interested in the crisis of the universities and student radicalism in the Weimar period will find Kater's book of value in spite of structural and analytical limitations. His research is careful and thorough. His treatment of the special character of the National Socialist students should suggest other specific studies of elements within the Nazi coalition.

MICHAEL STEINBERG  
Northwestern University

WOLFGANG STUMP. *Geschichte und Organisation der Zentrumspartei in Düsseldorf, 1917-1933*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 43.) Düsseldorf: Droste Verlag, 1971. Pp. 168. DM 42.

Despite the extreme social complexity of its electorate, the Catholic Center was the most stable of the German parties prior to World War I. But nearly forty percent of the voting Catholic population had been supporting other parties just before the war, and this monograph reveals that the Center had to struggle hard to secure substantial Catholic backing in urban industrial areas in the difficult times of the Weimar Republic. In Düsseldorf, whose population was approximately five-sevenths Catholic, the Center was able to win only a third of the Catholic vote in most elections; in fact, it would have done even less well if the newly enfranchised Catholic women had not tended to be more loyal to the Center than their husbands.

The author shows, however, that the Düsseldorf Centrists were resourceful in their efforts to keep their leading position in the city's council. They chose colleagues of high competence like Helene Weber, one of the first female Centrists, and Johann Giesberts, a distinguished laborite, to represent Düsseldorf in the Reichstag. They picked popular speakers to address election rallies, held frequent party assemblies, and, most importantly, created advisory councils for each of the professional and vocational groups in the Catholic electorate. But the Center's leaders at the national level believed that they could save their party only if they stressed programs of prime interest to the Church and those Catholics who had been affronted by the continued collaboration between the Center and the Social Democrats in Prussia. Wolfgang Stump may simplify things too much by implying that the primary emphasis on such pro-

grams led necessarily to the Center's acceptance of Hitler's Enabling Act of March 1933, but it is likely that the party could have realized its objectives only under some form of authoritarian government.

JOHN ZEENDER  
Catholic University of America

ALEXANDER KESSLER. *Der Jungdeutsche Orden in den Jahren der Entscheidung*. Volume 1, 1928-1930. (Beiträge zur Geschichte des Jungdeutschen Ordens, number 4.) 2d ed. Munich: Verlag Wolfgang Lohmüller, 1975. Pp. 176.

BARBARA SCHELLENBERGER. *Katholische Jugend und Drittes Reich: Eine Geschichte des Katholischen Jungmännerverbandes, 1933-1939, unter besonderer Berücksichtigung der Rheinprovinz*. (Veröffentlichungen der Kommission für Zeitgeschichte. Series B: Forschungen, volume 17.) Mainz: Matthias-Grünewald-Verlag, 1975. Pp. xxvii, 202.

The Weimar Republic was never able to gain the support of the majority of the German population. The left wing hoped to complete the abortive revolution of 1918; the right wing dreamt of restoring the monarchy and the authoritarianism of the Second Reich. A multitude of groups sprang up peddling remedies for the social and political dislocations of the day. Among them, appealing to the romantic nationalism of the Youth Movement and the self-sacrificing idealism of the trenches, was the Jungdeutsche Orden, led by Artur Mahraun. K. Hornung (1958) has already described its history in a scholarly fashion. Alexander Kessler's short study is the first part of a more detailed and more sympathetic account of the Orden during the years of crisis which led to the Third Reich. Drawn largely from the Orden's records and publications, it consists mainly of lengthy and somewhat repetitious extracts from the leaders' speeches and writings. Kessler is solely interested in the political activities and attitudes of the Orden, and provides no new insights into the social background or the organization of the movement's members. His day-to-day chronology is not entirely uncritical, and rightly points to the in-fighting which prevented the successful formation of any middle group capable of stopping the Nazi advance. But nostalgia motivates his writings and makes him unable to see the deeper reasons for the Orden's failure. He quotes, if only to deny, the not inappropriate judgment about Mahraun: "He was an imprecise, and indeed an impossible politician. His followers were worse. They hated Hitler. Was it only due to jealousy against the so much more successful rival 'völkisch' movement?"

Barbara Schellenberger's book follows closely on

the heels of Larry Walker's *Hitler Youth and Catholic Youth 1933-1936*. She claims to have consulted more original sources, particularly the diocesan records of the Rhineland where the Catholic youth movements were strongest. Her use of these sources is extremely detailed and thorough, again placing the chief accent on the conflict between the Nazis and the Catholic Youth Movement in the early years of the Nazi regime. The familiar apologetic tone recurs over the events of 1933 and the enthusiastic endorsement of the new era, followed by a hesitant realization of the Nazis' hostile intentions. Like their elders, German Catholic youth wanted to be good nationalists, and only the totalitarian demands of the Nazi radicals drove them into reluctant opposition. The author's useful analysis of the various Catholic youth groupings before 1933 is complemented by a full account of the measures of persecution suffered at Nazi hands. But these only confirm the judgment that such movements had themselves contributed to the downfall of parliamentary pluralism in Germany and assisted the Nazis to power. Despite the claim that the struggle for survival stimulated the resistance of conscience, the fact remains that, in strategy and tactics, the German Catholics were hopelessly outclassed. Apologias like these cannot disguise the fact that in the end German Catholic Youth and Jungdeutsche Orden alike willingly marched along with the rest to Hitler's orders in his wars of aggression.

JOHN S. CONWAY  
University of British Columbia

RONALD M. SMELSER. *The Sudeten Problem, 1933-1938: Volkstumspolitik and the Formulation of Nazi Foreign Policy*. Middletown, Conn.: Wesleyan University Press. 1975. Pp. x, 324. \$16.00.

MARTIN K. BACHSTEIN. *Wenzel Jaksch und die sudeten-deutsche Sozialdemokratie*. (Veröffentlichungen des Collegium Carolinum, volume 29.) Munich: R. Oldenbourg Verlag. 1974. Pp. 306. DM 44.

The Sudeten problem, which brought Europe to the brink of war in September 1938, spawned a vast secondary literature. Yet there are areas which have never been adequately served by historians. The complexity of the relationship between the formulation of Nazi foreign policy and the activities of Sudeten German nationalists left the way open to partisan accounts. These have obscured the image of the development by over-emphasizing the Nazi character of Sudeten German nationalism or by failing to consider the fact that gradually not the Foreign Office but the agencies of ethnic German politics shaped Reich policy toward Czechoslovakia and its Germans. The sharp clinical focus offered by Ronald M. Smelser on the

main Sudeten organizations and Reich German agencies involved in policy formation presents a highly suggestive account. The main thrust of his treatment, carried through chronologically, lies in the argument that far from being crypto-Nazis from the outset, Konrad Henlein and his aides were above all Greater Germans who only gradually became willing instruments of Hitler's aggression. The author sets Henlein and his movement firmly in the context of their time, and consequently offers the reader perhaps the most plausible political portrait of the Sudeten leader so far drawn by a historian.

Smelser quite correctly emphasizes the relevance of the Sudeten question to a broader understanding of the nature of the Nazi regime and general style of Hitler's policies. It seems, however, that he strives just a little too hard to attach every aspect of the Sudeten policies to Hitler's alleged pragmatism and indecisiveness. Certainly, the Führer did not prove to be amenable to being "educated" by Henlein (p. 241), nor could Himmler and the SS take charge of ethnic German affairs "without any effort" on Hitler's part (p. 210). Although the general character of the loose and fragmented Nazi power structure has been rightly stressed, Hitler has nevertheless been forced into a mold as a complete opportunist, a mold which he does not quite fit. But these are mere blemishes in an intelligent, informative, and well-researched book.

The subject of Bachstein's study is one of the main Sudeten opponents of Henlein, Wenzel Jaksch, a leader of the Sudeten German Social Democratic Party up to 1945. Clearly, an assessment of Jaksch's role during this critical period is much needed. Elected party chairman in March 1938, Jaksch advocated a non-Marxist brand of populist socialism closer to the Godesberg program than to the orthodox party tradition. As a spokesman of the German democratic minority he fought gallantly on the Czechoslovak side against Hitler but, curiously enough, he espoused "Greater German" views in his London exile after 1939. The work is arranged in two parts, the dividing point being the Munich crisis. Unfortunately, owing mainly to the absence of source materials, the first part appears to have been patched together rather than conceived as a whole; it is a compilation without benefit of illuminating arguments. The remainder of the study consists of a condensed but valuable account of Jaksch's relations with President Edvard Beneš in London.

Despite the efforts of the author in combing the sources, Jaksch as a man remains a shadowy figure. While he exists primarily as an essentially pragmatic politician, the paucity of personal reference has not been compensated for by information

on the party or by adequate historical focus. The author firmly planted his feet in the Sudeten camp, but his treatment of the Czech side is much less sure. Although the emphasis is heavily on Jaksch, the crises of his public career, such as his swift shift to Greater German ideas, are not well brought out. Nevertheless, despite its limitations, the volume gives a well-focused picture of Jaksch's role as political leader. Its usefulness is beyond question.

RADOMIR V. LUZA  
Tulane University

DETLEF JUNKER. *Der unteilbare Weltmarkt: Das ökonomische Interesse in der Aussenpolitik der USA 1933-1941*. (Stuttgarter Beiträge zur Geschichte und Politik, number 8.) Stuttgart: Ernst Klett Verlag. 1975. Pp. 397.

JAMES CLARK HUNT. *The People's Party in Württemberg and Southern Germany, 1890-1914: The Possibilities of Democratic Politics*. (Stuttgarter Beiträge zur Geschichte und Politik, number 9.) Stuttgart: Ernst Klett Verlag. 1975. Pp. 203.

A theme common to these disparate books is the failure of liberalism (broadly defined) to achieve its ends using liberal means. According to James Clark Hunt, the south German Populists were caught between authoritarian Prussianism on the right and Social Democracy and agrarian radicalism on the left. The *Volkspartei's* base of small-town and liberal farmers was narrow, yet cooperation with the right was inconsistent with its founding principles. On the other hand, its striving for parliamentary government and universal suffrage, in view of the emergent Social Democrats, was self-defeating.

The author's penetrating analysis concentrates first upon the party's social bases, leadership, goals, and fortunes on local, state, and Reich levels. Later he unravels its web of relations with the political forces of the Center and Left. Hunt concludes that the People's Party expended its effort on short-term coalitions with limited objectives and failed thereby to gain the governing experience necessary for later entry into national political power. This is consistent with most views of Wilhelmine liberalism.

According to Detlef Junker's study of economic interests and U.S. foreign policy, 1933-1941, U.S. policy also rested upon contradiction. Republican protectionism first dampened expectations of an open world market. Later, despite Cordell Hull's determined efforts, a rational free trade policy was incompatible with "enlightened" American interests and impossible in the context of representative government.

Foreign policy's defining characteristic, Junker asserts, was "globalism," the belief among New

Deal internationalists that the U.S. had inherited world-wide military, ideational, and economic interests best advanced through a global open-door policy, unencumbered trade, and multilateral most-favored-nation treaties. Threats to this indivisible world market in the form of Axis bilateral, nationalist, autarkical policies, he submits, helped bring the U.S. into World War II.

The author stands at variance with the Williams school, whose interpretations he finds rooted in monocausal, arbitrarily selected, unprovable, and *a priori* assumptions. Basing his view on his own earlier work on objectivity and value judgments in historiography (cf. *Historische Zeitschrift*, 211 [1972] 1-33 and *Beiheft* 3, *Neue Folge*, [1974], 1-46), Junker labels the New Left view a pre-Copernican, secular scholasticism that depends upon distinctions between "*causa prima*" and "*causae secundae*" (p. 230).

He sees economic interests playing an important but not determining role until about 1939 and an implementary function thereafter, explicitly rejecting the New Left belief that foreign policy at this juncture was a necessary reflex of the American liberal-capitalist system. Junker concentrates on such topics as the carryover of nineteenth-century economic doctrines; the "sham debate" over the New Deal as reform or revolution; bilateralism and the Axis; and anticipated threats to U.S. markets subsequent to an Axis victory over the European powers. His careful formulation rests upon extensive research in available American sources, printed material, and the *corpus* of European and American New Deal historiography. Rejection of monocausalism, though not new, allows him to argue a case based upon nuances, paradoxes, ironies, and contradictions: the vital stuff of substantial history that single-factor approaches either ignore, make soporific, or rationalize away. His conclusions, however, are buttressed by Irvine H. Anderson's recently published case study, *The Standard-Vacuum Oil Company and United States East Asian Policy, 1933-1941* (1975).

Both books contain extensive critical notes and selected bibliographies. Hunt's volume lacks an index, while Junker's contains the baleful *Personnenregister* that serves as its surrogate in many European publications. Hunt's first-class work helps complete the picture of Wilhelmine liberalism, while Junker's book, especially when translated, should stimulate lively and fruitful debate.

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TIMOTHY ALAN TILTON. *Nazism, Neo-Nazism, and the Peasantry*. (Indiana University Social Science Series, number 31.) Bloomington: Indiana University Press. 1975. Pp. xvi, 186. \$8.95.



Schleswig-Holstein continues to fascinate students of modern German politics. In 1945 Rudolf Heberle published his classic ecological study of the "distribution and constellation of social phenomena" there that led in July 1932 to the NSDAP's receiving its only state electoral majority before Hitler became chancellor. Timothy Alan Tilton's study is a continuation of Heberle's work (encouraged by Heberle himself) that seeks to explain why history did not repeat itself in the late 1960s when it appeared briefly as if Schleswig-Holstein might again become a regional base for a resurgent fascist-like party—this time the NPD.

In the state elections of 1967 the NPD received 5.8 percent of the vote in Schleswig-Holstein, well above the national average, but far less than the 51 percent gained by the NSDAP in July 1932. What had happened in the intervening decades to reduce so dramatically the reservoir of radical right-wing sentiment there? Tilton's thesis is that "the material substructure of politics in rural Schleswig-Holstein (and by implication, in West Germany) has undergone a fundamental alteration that has changed the social composition of rightist extremism and minimized its prospects." He argues that substructural changes in demography, ideology, economic structure, and political structure distinguish the Schleswig-Holstein of the late 1920s from that of the late 1960s and explain the NPD failure.

The originality of the study lies in the author's use of modernization theory, specifically that of Barrington Moore, who has located the social origins of fascist dictatorship in the failure of a society to transform its peasantry into a more "modern" social formation. Applying this framework, Tilton finds that it was not until the post-World War II period that Schleswig-Holstein underwent the structural changes that made it modern. Until that time, agriculture had been freed by tariff protection "from the unpleasant task of modernizing to meet foreign competition."

The analysis has considerable heuristic value for the study of rural political behavior in modern Germany. While statistical tables abound, one wishes the model had been filled out more completely by detailed research into German economic and social history.

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JILL STEPHENSON. *Women in Nazi Society*. New York: Barnes and Noble, 1976. Pp. 223. \$17.50.

Fascism has always had a horrific fascination for historians and currently enjoys a new vogue for

reasons best known to future historiographers. Part of its peculiar attraction and repulsion has been its strident machismo, its very caricature of gender differences. Therefore a serious study such as Jill Stephenson's provides a sobering antidote to the myth of the Nazi's unique sexism.

Using a wealth of documentation from government archives, newspapers, and personal sources, she proves considerable continuity in the position of women in German society from the Weimar Republic to World War II. Hitler's emphasis on women's reproductive duties affected their real lives little more than had the republic's pretense of furthering their equality with men. Ironically, the Nazis' urgency to promote childbearing led to policies contradicting their polemic glorifying the family. They eased the divorce law and improved the status of unmarried mothers, changes championed by the women's movement they destroyed. These changes had not previously been instituted by the republic, thanks mainly to the strength of the Catholic Center party in its coalition governments.

Stephenson shows that in women's employment the Nazis simply adjusted to economic depression and recovery. During the Depression they removed some women from the work force by offering marriage loans on the one hand and restricting women's employment on the other. Recovery was facilitated by Hitler's anticipation of Keynesian economics in the form of state expenditures on armaments and public works. New jobs thus created relieved male unemployment, reduced job competition, and allowed women to flow back into the labor force. World War II, for Nazi Germany as for its adversaries, drew again on women's reserve pool of labor. Stephenson also points out that in higher schooling and the professions the Nazi government carried forward many educational policies prefigured in the last years of the Weimar Republic. It cut back university enrollment and reduced the proportion of women in it to counter the growth of an unemployed intellectual elite and to eliminate competition within that group. When the need for professional services again expanded, women resumed their "normal" proportions in the professions, even advancing them in some cases.

While *Women in Nazi Society* is meticulously researched, if a bit bloodless for lack of contemporary personal testimony, it falls short in the realm of interpretation. The author tends toward monocausality and credulity in ascribing so many aspects of Nazi policy to their well-publicized rationale of raising the birth rate. Such naiveté is surprising in view of her own data. Sheer hypocrisy, not idealism mitigated by pragmatism, accounts for the Nazis' sentimental concern for women's physical disabilities in industrial work



when unemployment was rife and their simultaneous channeling of women into far more strenuous and exploitative "voluntary" rural labor and domestic service. The differential treatment accorded middle-class women with a servant shortage and working-class women with a job shortage reveals the hollowness of the *Volk* mythology with its class-consensual implications. But Stephenson seems willing to accept Nazi protestations of concern for preserving treasured national values like housekeeping skills and female attributes of service and sacrifice. On balance the book is "soft on Nazis," the author's introductory disclaimer notwithstanding. This may be because Stephenson read her Nazi sources too uncritically. For example, labor historians will be startled to read, on page 108, that during the war managers of firms demanded equal pay for equal work performed by women. The footnote refers to a report from one of Himmler's security agents—a questionable source at best for such an assertion.

Not only is Stephenson's willingness blandly to credit Nazi sincerity disturbing, but her analysis of the total context is superficial. Thus, while her material neatly proves continuity between the Weimar Republic and Hitler Germany, as well as similarities between the latter and non-Fascist countries, she fails to draw the obvious conclusions. The Nazis did not dismantle capitalism. Their policies on women, like those of their allies and adversaries, traced out the shape of the economic system and the vagaries of its business cycle. The Nazis were not alone in restricting women's employment and in sponsoring pronatalist policies during the depression and then enlisting women on the home front with war and economic recovery. Political ideology seems to have been a surface manifestation; the ships of state only sailed, they did not make these waves.

A major reason for the error is the author's time frame. The period from 1930 to 1940 is largely a politically defined decade that masks more profound structural changes with a different chronology. Thus, the choice omits major transformations of 1925 to 1929 in the German economy, such as rationalization of industry with its new and controversial sexual division of labor. The surges of industry are a vital dimension for any understanding of women's position in society as producer, reproducer, and consumer.

Still, Stephenson has mined her sources well. She has extracted valuable ore for us to refine a bit further.

RENATE BRIDENTHAL  
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JOACHIM HOFFMANN. *Die Ostlegionen, 1941-1943: Turkotataren, Kaukasier und Wolgafinnen im deutschen*

*Heer*. (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, 19.) Freiburg: Verlag Rombach. 1976.

By 1943, approximately 200,000 Soviet citizens, mostly former prisoners of war, were serving in the so-called "Eastern Legions" of the German army. Unlike the men of the better-known Vlassov army, these were individuals of non-Slavic nationality, primarily members of Turkic-speaking nationalities, but including Georgians, Armenians, and others as well.

Joachim Hoffmann's pioneering work deals with the creation, outfitting, and preparation for battle of the Eastern Legions; a companion volume will deal with their combat role. Hoffmann's chief goal is to show how an "amorphous mass" of Red Army prisoners was transformed into a fighting force, and he makes it clear that the original motivation of the legionnaires was to escape the intolerable conditions of the POW camps. Well over half the non-Slavic prisoners died during the winter of 1941-42, many of them shot by the extermination squads of the *Einsatztruppen* as racially alien "Mongols" or even "Jews."

The most interesting and valuable part of the book is Hoffmann's discussion of German propaganda efforts within the legions. The Germans soon realized that, because of the effects of Soviet indoctrination, simple-minded anti-Bolshevik propaganda would be useless. They therefore concentrated, with some success, on three themes: Soviet suppression of religion (especially effective with Muslims), the superior standard of living in Germany, and national self-determination under the German aegis.

Hoffmann is best at detailing the development of the legions, but fails to tie this in with the larger question of German policy toward occupied Russia. The narrow focus of this study leaves the impression that the German army in Russia formulated and implemented policies which would have determined the future of the Caucasus region and Central Asia had Germany won the war.

MICHAEL G. HILLINGER  
*Hampton Institute*

MARTIN SCHÄRER. *Deutsche Annexionspolitik im Westen: Die Wiedereingliederung Eupen-Malmedys im zweiten Weltkrieg*. (European University Papers. Series 3: History, paleography and numismatics, volume 38.) Bern: Herbert Lang. 1975. Pp. 359.

This penetrating monograph (complete with documentation, maps, and illustrations) by a young Swiss scholar relies heavily on extant German and local records as well as on contemporary published material to describe in detail the German annexation and administration of certain Belgian terri-

stories during World War II. Although the area involved—which included the Eupen and Malmedy districts ceded by Germany to Belgium after World War I as well as a number of traditionally Belgian, but German-speaking villages—was relatively small (about 89,000 inhabitants), the work makes a significant contribution as a case study in the field of Nazi territorial and ethnic policies, which emerge here as a hodgepodge of economic aid, military exploitation, red tape, racial delusion, and security paranoia. Most of the inhabitants were made German citizens and were subjected to conscription to the Wehrmacht, but for apparently bureaucratic reasons their Belgian citizenship was not abrogated. They were also denied, as a rule, appointment to positions of responsibility. The Reich was generous in facilitating economic integration, and Nazi leaders never understood the opposition of the entire population of the few villages that had been traditionally Belgian. Moreover, in regard to the former German population (which had been transferred under the Versailles settlement) Schäfer concludes that Nazi annexation policies actually strengthened their pro-Belgian attitudes.

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W. W. KULSKI. *Germany and Poland: From War to Peaceful Relations*. Syracuse: Syracuse University Press. 1976. Pp. xxii, 336. Cloth \$22.50, paper \$9.95.

For twenty-five years after World War II, the Polish-German frontier remained—after the division of Germany—the most striking European territorial problem resulting from the war. Poland's "recovered territories" were carved out of eastern Germany in 1945 in compensation for the Soviet annexation of eastern Poland, but the question whether Poland would be able to retain control of this area long remained an international controversy. The official Polish position, as Gomulka frequently stated, was clear: "If anyone puts forward the question of changing our western frontier, there is only one alternative—the alternative of war." On the other hand, until recently West Germany officially considered the Oder-Neisse frontier a provisional one, pending a general European peace treaty. As Adenauer stated in 1949: "We shall not stop voicing in an orderly legal way our claims to these territories."

W. W. Kulski competently treats the question of contemporary Polish-German relations in his latest book, in which he successfully depicts "the successive stages in the development of relations between the Federal Republic of Germany and Poland up to the present time" (p. xiii). (The book

deals only parenthetically with the German Democratic Republic.)

As a former (1928–45) Polish diplomat who was able to observe the successive German policies toward Poland as well as a scholar who knows the Polish and German source materials intimately, Kulski is frankly sympathetic to the Polish side of the controversy. His is, however, a mellow kind of commitment, as indicated by his implicit—and at times explicit—support of German-Polish "reconciliation." In fact, one of the most interesting aspects of the book concerns the growing differentiation of German public opinion toward the "Polish question." The upshot of this is Bonn's recognition of the Oder-Neisse frontier, leading Kulski to conclude on an optimistic note. He sees improving relations between West Germany and Poland and places his major hope upon the younger generation of both nations for an end to the hatreds of the past.

I recommend this volume to the general reader as well as to the specialist.

HARRY E. DEMBKOWSKI  
Alliance College

DOMENICO MASELLI. *Tra risveglio e millennio: Storia delle chiese cristiane dei fratelli, 1836–1886*. (Storia del Movimento Evangelico in Italia, number 3.) Turin: Editrice Claudiana. 1974. Pp. 330.

This useful but highly specialized study traces in chronological fashion the tribulations of evangelical Protestants during the Risorgimento. The author is never less than understanding of, and occasionally is more than sympathetic toward, the plight of the evangelical movement or, more accurately, a series of not always connected movements. The success of these movements did not involve large numbers, and their role in fostering religious toleration in Italy, notwithstanding the author's claims for an important educational role, cannot be explained except as a small irritant in the conflict between spiritual and secular Rome. By giving due attention to regional variations, Domenico Maselli accurately captures the isolated community and even cellular character of the evangelical millennialists he studies. Less fortunately, this gives his treatment a sporadic quality and sometimes causes him to pass too quickly over folk responses, as in his assertion that the movement fared badly in Puglia because of local superstitiousness and the belief that Protestants were classical bearers of the evil eye.

Readers who wish to learn about persons, organizations, and events connected with evangelicalism in nineteenth-century Italy surely will consult this thoroughly documented book, one in

which the author's sympathies, though evident, do not compromise his presentation of factual material. His analysis of the decline of millennial-evangelical fervor is for the most part persuasive: the deaths of Piero Guicciardini and Pietrocola Rossetti denied to the movement its most powerful spokesmen and organizers; the legal, if not social and cultural, acceptance of religious toleration, robbed potential converts of that zeal which feeds upon persecution; and the takeover of the movement by English forces (whence it had originated a century earlier), who badly misunderstood Italian cultural needs, alienated many followers.

But Maselli's most interesting hypothesis, one hardly less important for remaining unproven, is that the movement's primary appeal always had been to peasants and especially to urban workers and that its decline was spurred by agitation arising out of the First International and the development of an organized proletarian consciousness. Eric Hobsbawm, in *Primitive Rebels*, brought to wide attention a related argument in his account of Tuscany's Lazzarettists. Any temptation to dismiss such evidence of "religio-political millenarianism" as quaint and fragmentary will be overcome by readers of Maselli's volume. This reviewer finds at least a kernel of truth in the author's assertion (p. 65) that evangelical millenarianism was "born for the most part among field laborers, factory workers, in reality all the poorer classes, who disseminated a new faith in which they saw hope for greater freedom and a richer spiritual life."

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RICCARDO FAUCCI. *Finanza, amministrazione e pensiero economico: Il caso della contabilità di stato da Cavour al fascismo*. (Studi, number 20.) Turin: Fondazione Luigi Einaudi. 1975. Pp. 209. L. 3,000.

Written with "today's situation" in mind, this study concentrates on the evolution of the machinery for preparing and administering government budgets in Italy from national unification to the fall of the Fascist regime. Implicit in the author's presentation is the notion that, far from being of a transient nature, the problems that plague the bureaucracy today are the result of deeply rooted historical realities. Thanks to this broad historical perspective, this specialized study has much to say to a large public curious about the ways of bureaucracy. Students of modern Italy will benefit by seeing such well-known phenomena as *trasformismo*, Giolittian democracy, and Fascism treated

from an unusual perspective of administrative thought and reforms.

Initially assigned the straightforward task of insuring that public revenues were legally allocated and spent, the agencies responsible for the national budget have since been called upon to assume additional major responsibilities, such as monitoring the efficiency of public spending, choosing between alternate spending strategies, allocating resources among competing interest groups, and stimulating economic growth. Not surprisingly, greater responsibilities have brought greater power to the bureaucracy. Greater power has in turn made the bureaucracy more pervasive, obtrusive, and independent of the legislative branch whose proper task is to direct administrators in their duties.

The author is particularly skillful when he traces, with abundant documentation, the changing relationship between the executive and legislative branches of parliament which comprise respectively the bureaucratic and political leaders in government. Civil servants and political leaders were not always clearly distinct groups, especially in the thirty years after national unification when academic, civil service, and political figures exchanged roles with relative frequency. It was only in the Giolittian era (1900-14) that the lines between civil service and political careers were drawn more clearly, and civil servants came to think of themselves as the guarantors of competence and stability in government against the polemics and mutability of party politics. The Fascist years gave the bureaucracy an even stronger sense of its own corporate identity. The well-known reforms of 1926, for instance, authorized executive agencies to issue legally binding regulations without first consulting with legislative bodies. Though the study ends with the downfall of Mussolini in 1943, the author makes clear that the vexing problem of an unmanageable bureaucracy which confronts today's leaders had emerged fully more than thirty years ago. The implicit message is that no meaningful reform of the bureaucracy is possible in the absence of purposeful political leadership and without a return to a fundamental division of functions between politicians and civil servants by which the former legislate and the latter administer the legislation.

ROLAND SARTI  
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Amherst

CARLA MENEGUZZI ROSTAGNI, editor. *Il carteggio Antonelli-Barili, 1859-1861*. (Istituto per la Storia del Risorgimento Italiano. Biblioteca scientifica. Sec-

ond series: Fonti, volume 65.) Rome: the Istituto. 1973. Pp. xxxii, 420.

This collection of documents, ably edited by Carla Meneguzzi Rostagni, comprises the correspondence between Cardinal Giacomo Antonelli, Papal Secretary of State, and Monsignor Lorenzo Barili, Papal Nuncio at Madrid, relative to the Italian unification movement of 1859-61. Though a second-rate power in the community of nations, Spain ranked as one of the foremost Catholic countries of the world, and Antonelli and Barili hoped to use the Spanish against Sardinian encroachments upon papal territory.

The documents indicate that Spain followed the events taking place in the Italian peninsula with keen interest, not only because of the involvement of the Holy See, but also because of its dynastic connections with Parma and Naples. Barili sought the endorsement of the Spanish government for Cardinal Antonelli's proposed congress of Catholic powers, the purpose of which would be the preservation of the integrity of the Papal States.

The Barili-Antonelli efforts to obtain Spanish support ended in failure. Though Queen Isabella was sympathetic to the idea of armed force for the defense of the papacy, the ministry of Leopoldo O'Donnell decided on a policy of neutrality. The internal situation was not conducive to intervention in the Italian peninsula. Furthermore, Spain feared antagonizing France, whose support it needed for the Moroccan campaign that was about to be launched.

The letters of Barili contain a wealth of information concerning Spain during these years. The nuncio was a conscientious observer, and he evidently enjoyed writing. In response to Antonelli's brief and laconic letters, Barili replied at great length, giving not only factual data, but his own personal impressions. Almost all aspects of Spanish life—politics, the economy, the social scene—were noted and often minutely analyzed by the monsignor.

The documents in this volume are derived from the Archivio Segreto Vaticano, particularly the archives of the Secretariat of State and the Nunciature of Madrid. Students of papal or Spanish diplomacy during the years 1859-61 should find these documents extremely valuable.

ELISA CARRILLO  
Marymount College

ANTONIO CONFALONIERI. *Banca e industria in Italia, 1894-1906. Volume 1, Le premesse: Dall'abolizione del corso forzoso alla caduta del Credito Mobiliare.* (Studi e ricerche di storia economica italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana. 1974. Pp. 453.

This is another in the series of studies published by the Banca Commerciale Italiana on the economic history of Italy from the dawn of the eighteenth century to the World War. Confalonieri's volume concentrates upon economic developments from the abolition of the inconvertibility of banknotes decreed in 1881 to the collapse of the Credito Mobiliare twelve years later. It contains much about Italian banking and less, by comparison, about Italian industry during these years, although the role of banking in the country's industrial expansion is examined, and there is a section on the successful operation to rescue the Terni steelworks from bankruptcy.

The author argues convincingly that the termination of the *corso forzoso*, which theoretically made the paper money of the banks of issue convertible into gold and silver, was a political decision which was premature if not mistaken. He shows that this reform, which was supposed to restore Italy's fiscal elasticity, brought a wave of inflation, unrestrained speculation in the building industry, a rapid expansion of the railway network, monetary crisis, and eventually economic stagnation. Within this framework the collapse in 1893 of the two largest credit institutes, the Credito Mobiliare and the Banca Generale, appears as the culmination of this tormented period. At the same time the consequences of the abolition contributed to the crisis of the banks of issue, and in particular the Banca Nazionale, which sacrificed its prestige and resources to bolster a number of banks and industrial enterprises.

All this is well documented by a wide variety of sources drawn from government and bank archives and is further supported and clarified by copious footnotes and a statistical appendix which includes twenty-two tables containing useful information. There are an additional thirty-one tables interspersed within the text, but these unfortunately do not have an index, although there is an index of names of persons, another of individual enterprises and companies, and a general index of subject matter included within the volume.

The material found herein will be appreciated by those interested in the economic life of Italy and particularly the relationship between banking and industry in the age of Depretis and Crispi. The final chapter broadens the scope of the work somewhat by tracing the formation of the Italian banking tradition within the framework of the banking experience in continental Europe. It is a book the specialist will have to consult, despite the fact that its organization lends itself to considerable repetition.

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PIER GIORGIO ZUNINO. *La questione cattolica nella sinistra italiana (1919-1939)*. Bologna: Il Mulino. 1975. Pp. 503.

Conducive to a better understanding of the Italian political scene during and after World War II is this study of the Italian Left from 1919 to 1939. Pier Giorgio Zunino, Professor of Contemporary History at the University of Turin, has undertaken an exhaustive research of the available sources (printed and archival) on the Italian Socialist Party, the Italian Communist Party, the Antifascist Concentration, and the Justice and Liberty movement insofar as these parties and movements concerned themselves with Catholicism. The result is a well-documented and generally objective analysis of a controversial subject. Although the author's prose is somewhat tedious, the book makes fascinating reading.

Zunino points out that the emergence of the Popular Party was initially greeted with hostility by the Italian Left. The growth and eventual victory of Fascism led Italian socialism to reconsider its attitude toward Sturzo's party. If no alliance between socialism and Popularism developed, the negative attitude of the Vatican was primarily responsible. During the years when Socialist and former Popularist leaders were in exile, relations between the two groups fluctuated. Prior to the Lateran Pacts, socialists were disposed to conduct dialogues with Catholics. The Conciliation of 1929 and the Church's support of Mussolini's Ethiopian war brought estrangement.

The most interesting of the ten chapters that make up this book are those dealing with the Italian Communist Party (PCI). According to Zunino, the PCI initially viewed Popularism as a form of social democracy that would lead to the disintegration of the Church as it entered the temporal arena. When the Vatican dissociated itself from the Popular Party, the PCI saw an opportunity to radicalize Catholic peasant and worker organizations. Regarding Catholicism, communist leaders such as Gramsci and Togliatti taught that the masses could be permitted to practice their religion; only those who entered the Communist Party would be expected to give exclusive allegiance to materialistic Marxism. Efforts to infiltrate Catholic ranks were generally unsuccessful, even though communism had won over such a powerful peasant leader as Guido Miglioli.

After 1934 the policy of attempting to penetrate Catholic ranks was abandoned in favor of the *mano stesa* (outstretched hand). Communist leaders followed with increasing interest manifestations of anti-Fascist attitudes and movements among Italian Catholics, such as the Neo-Guelfs. The antagonism that developed between the Vatican and Nazi

Germany, as well as the Church's resistance to Italian racialist legislation, seemed to justify the policy of the *mano stesa*. Communists went so far as to state that Fascist attacks on Catholic Action were tantamount to attacks on the entire Italian people. When Pius XI died, the PCI praised him as the enemy of plutocratic capitalism and totalitarian barbarism. The election of Cardinal Pacelli was hailed as a victory for peace. The PCI's enthusiasm for Pius XII soon dissipated, however, and the party's policy on the eve of World War II was more than ever subordinate to Moscow, which was about to sign the Molotov-Ribbentrop Pact.

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ALBERTO AQUARONE and MAURIZIO VERNASSA, editors. *Il regime fascista*. (Serie di storia.) Bologna: Il Mulino. 1974. Pp. 527. L. 6,000.

As the two editors of this anthology correctly point out, after the Second World War most studies on Fascism tended to concentrate either on its origins in Italy or on whether Fascism should be considered a peculiarly Italian phenomenon rather than the symptom of a world-wide crisis, the result of a deep twentieth-century malaise. The actual functioning of the regime, its internal dynamics, the dialectical interplay among the various economic and social forces within Italy were not investigated. In the early 1960s, when the passions that Fascism had left as its legacy tended to subside, Delio Cantimori wrote that historians should stop considering Fascism as "a whale that swallowed everything indiscriminately. . . . It is necessary to distinguish the variety of currents, movements, tendencies, persons, economic and financial interests . . . and even illustrate the illusions [and] fantasies . . . that permitted Mussolini and his followers to conquer power and keep it" (*Conversando di storia* [Bari, 1967], p. 134). Following his advice, many Italian historians in the last decade have started to look at discrete aspects of the regime. Alberto Aquarone's *L'organizzazione dello Stato fascista* (Turin, 1966) was one of the first such works.

In this anthology, the editors have brought together seventeen articles, all previously published in scattered journals or other collections. They preferred not to use excerpts from longer works in order to provide the reader not with snippets, but with self-contained pieces of research and writing. Each deals in some detail with a different segment or development in the Fascist period. Organized in three parts, the book looks at the political organization of Fascism, its economic system, and its foreign policy. Among the contributors are established scholars like Renzo De Felice, Giorgio



Rochat, Pietro Scoppola, Paolo Ungari, Giam-piero Carocci, Giorgio Rumi, and Aquarone himself, and younger historians and economists. It is difficult to single out any one article for particular discussion, as they are all excellent on the specific topic discussed by the particular author, and space precludes a review of all seventeen. Of perhaps particular interest, since they touch on less-known aspects of the period, are the articles by Gian Carlo Jocteau on the functioning of the labor courts from 1926 to 1933, Piero Calamandrei's contribution on the role of parliament, and Paul Corner's study of the relations between agriculture and industry under Fascism.

The book as a whole makes no pretense at presenting an organized and clearly defined interpretation of Fascism. In the words of the editors it investigates "the structures and . . . the functioning of the Fascist regime as it developed concretely in Italy" (p. 25). Like the collection of essays edited by Roland Sarti, *The Ax Within: Italian Fascism in Action* (New York, 1974), its purpose is to offer critical observations rather than preconceived interpretations, to raise questions rather than to provide final answers, to indicate areas where further research is needed. An up-to-date bibliography of the literature on Fascism at the end of the book and the footnote references to each article add to its scholarly value and usefulness. Even though students of Fascism who try to follow the literature may be familiar with the articles included in this anthology, it is useful to have them easily available and to see the connecting historiographical thread among them. For those who may have missed many of the essays included, this collection will provide access to some important contributions to the study of Italian Fascism.

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CLAUDIO G. SEGRÈ. *Fourth Shore: The Italian Colonization of Libya*. (Studies in Imperialism.) Chicago: University of Chicago Press. 1975. Pp. xix, 237. \$15.00.

This book, a revision and expansion of Claudio G. Segrè's doctoral dissertation, is a work of original scholarship based largely on archival research in Italy at the Central Archives of the State, the now defunct Ministry of Italian Africa, the Foreign Ministry, and the important institutes of tropical agronomy. His research was supplemented by interviews with Italians who had once been settlers in Libya, and it also covers the whole range of secondary materials on the subject. Although *Fourth Shore* lacks an investigation of Arabic-language sources, this is not a grave methodological

or substantive weakness in view of the author's thesis.

Segrè sets out essentially to study how Italy got involved in Libya through an extension of the pre-Fascist concern with overpopulation and the consequent emigration of vast numbers of impoverished Italian peasants to lands where they were often poorly treated. Abandoning his once strong conviction that imperialism was inappropriate for Italy, Mussolini revived the imperial drive of his Liberal predecessor, not for demographic reasons, but for purposes of glorifying the Italian state, rediscovering the heritage of Rome, revivifying an allegedly soft Italian people, and seeking revenge for defeats in Africa at the end of the nineteenth century.

Initially the Fascist state encouraged colonization through private concessions alone, but large-scale capital was not responsive to the challenge and small-scale settlers and their families were either inadequate to the task or not attracted to the North African littoral. Subsequently, as Mussolini reversed the demographic concern for overpopulation and developed his mania for fertility, large families, and a growing Italian population that would remain under the Italian tricolor, Libya grew in importance as a source of land under Italian sovereignty. The culmination of these settlement schemes came during the governorship of the flamboyant and charismatic Italo Balbo, whose tremendous drive and remarkable organizing skills led to the mass settlement of twenty thousand Italian colonists within the space of a few months in 1938.

The Second World War brought an end to Italian colonization plans in North Africa, just as the Fourth Shore seemed about to become a reality. One hundred thousand Italians found themselves stranded in Libya as the country became successively a battlefield, a pawn in the postwar deliberations of the great powers, and finally an independent state hostile to the presence of Italian colonists on land alienated from Libyan Arabs. In 1970 the few who had chosen not to be repatriated were driven from the country by its zealous and xenophobic government, thus ending the sixty-year presence of Italians on their erstwhile Fourth Shore.

Segrè has thoroughly researched and lucidly presented his findings. His work is of importance to three groups of specialists—those interested in Italian history, those concerned with the history of imperialism and colonialism, and those involved in the general area of African studies. Its particular appeal will probably be to the first two groups, for the "African" content of the book is slim indeed (although there is a rather good chapter on Libyan

perspectives on the Italian undertaking). This does not detract from the work, however, for its major contribution is clearly in European and colonial studies. In an important sense this book fills a gap in our knowledge, for virtually nothing has been published on the subject in English, while the Italian literature on the topic is polemical or apologetic and lacks the objectivity that one finds throughout Segrè's book.

The book will also find a wider audience in those interested in crosscultural contact and the coexistence of mutually alien societies. Especially fascinating in this regard is Italy's attempt to recreate in a North African colony a European peasant culture; this is in strong contrast to all other European developmental schemes for African possessions, Portugal excepted. The role of the technocrat, in this case specialists in tropical agriculture, is equally intriguing. Segrè deftly explores this, thus suggesting that other authors might take into consideration a new dimension in the study of colonial settlement experiments.

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Chicago Circle

JOHN C. CAMPBELL, editor. *Successful Negotiation: Trieste 1954, An Appraisal by the Five Participants*. Princeton: Princeton University Press. 1976. Pp. viii, 181. \$11.50.

This brief study of how the thorny issue of the Italo-Yugoslav border was resolved is unique. For the most part it is told in the words of the participant negotiators (U.S., British, Yugoslav, Italian) in the form of skillfully conducted interviews. The editor, long a State Department expert on Eastern Europe, was especially well-qualified to do this study. In addition to the interviews, he provides a succinct introduction to the problem and concludes with an analytical summation.

The Trieste settlement was made possible by a happy combination of circumstances. Both sides were ready for compromise, realizing that the most they could get (with minor modifications) was a division of the territory along the lines of Zone A and Zone B. The negotiators were able and experienced, and they were not hurried, and, most important, they were able to proceed in secrecy. Moreover, the U.S.-British declaration that they intended to turn over the occupation of Zone A to the Italians provided the catalyst to get the negotiations started.

Although legally provisional, the settlement seems permanent and therefore an unqualified success. The editor raises the question of whether other international disputes (for example, the

Arab-Israeli struggle) might lend themselves to similar negotiating tactics and concludes that the answer must remain uncertain and problematical.

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GEORGIOS PSYLLAS. *Apomnēmoneumata tou biou mou*. [Recollections of My Life]. Edited by E. G. PREVELAKIS (Akademia Athēnōn. Kentron Ereunēs tēs Historias tou Neōterou Hellēnismou. Mnēmeia tes Hellēnikēs Historias, 8.) Athens: the Akademia. 1974. Pp. 370.

This volume is a handsome and welcome addition to what has long been a substantial body of published memoir literature on the Greek war of independence and the early decades of modern Greek statehood. George Psyllas (1794-1878), a prominent public figure of "the generation of 1821," was a Western-educated native of Athens identified with the "English" party. With the aid of a personal diary (of which there is unfortunately no longer any trace), he wrote his memoirs at an advanced age, mainly between May 1867 and September 1869, for his children rather than for publication. The manuscript remained in the private possession of his heirs, accessible only to a few scholars, until sold in 1970 to the General State Archives of Greece. With the exception of a few fragments, it remained unpublished until the Academy of Athens commissioned its Research Center for the History of Modern Hellenism to prepare the entire manuscript for publication as the eighth of its current series of source materials entitled "Monuments of Greek History." The result is a model of professional editing and thorough scholarship, such as anyone familiar with the publications of the Center, under the directorship of Eleutherios G. Prevelakis, has come to expect of this distinguished historian and his able staff.

The editor has divided the manuscript into fourteen chapters. Orthographical and other emendations of the text are made for the reader's convenience but are kept to an absolute minimum and always indicated. Otherwise, the integrity of the original text is respected and its pagination indicated in the margins. The editor's introduction succinctly but fully describes the original manuscript—its physical characteristics, history, contents, and style—and explains the editorial format employed in its publication. Apart from identifying textual idiosyncracies and editorial emendations, footnotes cite documents and date events referred to in the text. An excellent bibliography and index complete the volume, which a member of the Academy introduces on behalf of its members. Nikolaos Louros, a distinguished scholar,

teacher, and practitioner of obstetrics, briefly outlines the career of Psyllas and evaluates the contents of the memoirs.

In many respects the memoirs prove disappointing. They confirm the already established image of Psyllas as a man of principle, a liberal constitutionalist, and both a practitioner as well as a defender of free expression, but they never reveal the inner man or the more intimate aspects of his private life. Nor do they often give more than an outer view of national events that, through active participation, he knew intimately from the inside. On the other hand, the memoirs prove more interesting and revealing in unexpected areas, such as the Greek educational system in Athens during the last two decades of Ottoman rule there, municipal life in Athens and conditions in provinces where Psyllas served briefly as provincial governor, and the tangled question of property rights and litigation over these as Greece made the transition from Ottoman to Western legal codes. Though the memoirs provide nothing startlingly new for modern Greek historiography, they provide a variety of information and a point of view which, if used with other sources, can shed light on issues of modern Greek history that still remain open.

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ROBERT F. BYRNES, editor, *Communal Families in the Balkans: The Zadruga. Essays by Philip E. Mosely and Essays in His Honor*. With an introduction by MARGARET MEAD. Notre Dame: University of Notre Dame Press. Pp. xxvii, 285.

WAYNE S. VUCINICH. *A Study in Social Survival: The Katun in Bilëa Rudine*. (Monograph Series in World Affairs, volume 13, book 1.) Denver: University of Denver. 1975. Pp. xxiv, 194. \$2.50.

Philip Mosely, the late Columbia University historian, is honored in this volume not so much for his outstanding contributions in international relations, foreign policy, and the history of Russia and the Soviet Union, but for his pioneering work on the South Slav *zadruga*, or communal family. Articles by Margaret Mead, Leonard Schapiro, and Stavro Skendi provide details about Mosely's career, his warm, humane relations with colleagues and students, and his ability to bridge disciplines in search for answers to problems that interested him. An important contribution of this work is the combination in one volume of the four seminal, well-documented, and carefully reasoned articles that Mosely wrote on the *zadruga* and the Russian family, together with a full bibliography of Mosely's publications. Of interest, too, are articles on the *zadruga* by Eugene Hammel, Olivera Burić, Wayne Vucinich, Jozo Tomasevich, Ante Kadić, David Rheubottom, C. J. Grossmith, Vera St. Er-

lich, and Emile Secard. Daniel Chirot contrasts the Romanian communal village with the *zadruga*. Robert Byrnes deserves much credit for organizing the 1973 conference dedicated to the scholarly work of Mosely, for which the papers in this volume were prepared, and for the unity of the published volume itself.

Vucinich's study draws useful distinctions between the *katun*, or pastoral community, in the Middle Ages and today. The early form was based on kinship, whereas today it is a summer association made up of compatible households not necessarily related by blood. The essential feature throughout has been life with the stock in the mountains, where animal breeding and milk processing techniques have been primary concerns. Vucinich has made his way through a maze of documentary material so as to pose the proper questions, such as what is the connection between the *katun* and the Latin-speaking Vlach or its relationship to the *zadruga*. This work is the first of four essays on East European themes that will constitute a volume in the Monograph Series in World Affairs (University of Denver, vol. 13 [1975-76]), dedicated to Josef Korbel, who developed the Graduate School of International Studies and until recently served as its dean. When he chose to leave a distinguished career in the Czechoslovak diplomatic service and seek citizenship in the United States, Philip Mosely made the telephone call to the University of Denver that led to Korbel's appointment there.

I have had the good fortune to know both men: Philip Mosely in the 1930s in the Balkans while doing research on Bulgarian village life and Josef Korbel in the early 1950s. It is fitting that two books in their honor should be those that add significantly to our knowledge of the *zadruga* and the *katun*.

IRWIN T. SANDERS  
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RĂZVAN THEODORESCU. *Bizant, Balcani, Occident la Începuturile Culturii Medievale Românești (Secolele X-XIV)*. (Biblioteca Istorică, number 41.) Bucarest: Editura Academiei Republicii Socialiste România. 1974. Pp. 379.

Răzvan Theodorescu has set for himself the complex task of isolating and analyzing the main sources of Romanian medieval culture and of placing that culture in a European context. This is a work of synthesis, which draws critically on the theories and discoveries of several generations of archeologists and cultural historians from the nineteenth-century romantic polymath Bogdan Petriceicu Hasdeu to the present. Theodorescu is at home in a variety of disciplines—the history of art and architecture, literature, folklore, political

and ecclesiastical history—all of which he uses in order to lay as broad a base as possible for his investigation. Geographically, he is concerned with the Lower Danube, particularly its eastern (the Dobrudja) and western (the Banat and Oltenia) extremities.

After establishing the relationship between the rural and folkloric culture of the fourth to ninth centuries and the later centuries of conscious creativity, he devotes the bulk of his narrative to the "mature" phase of Romanian medieval culture of the thirteenth and fourteenth centuries. He considers the formation of the principalities of Moldavia and Wallachia, a process he treats in detail, as a decisive event; the two states provided the proper environment within which the theological doctrines and the organized church of the Byzantine-Balkan world, the main determinants of mentality and sensibility in the medieval East as in the West, could express themselves. Theodorescu then proceeds to examine Roman Catholic missionary activity and the more important work of ecclesiastical organization carried out by the Orthodox clergy, and to show how the popular culture and patriarchal religious life of earlier centuries gave way to the norms imposed by Constantinople and a burgeoning native feudal order. Finally, two long chapters are devoted to the influence of theological doctrines and other spiritual currents emanating from the Balkan church (there is a penetrating account of Romanian monasticism) and to the reflection in art and architecture of the evolution of Romanian lay and ecclesiastical society in the fourteenth century.

The author argues convincingly that the Lower Danube forms a separate cultural zone between the Byzantine-Balkan world to the south and Central Europe, represented by Catholic Hungary, to the north. Yet it is evident that Eastern contacts and influences were paramount in molding Romanian medieval culture, and the author would certainly agree with Dimitri Obolensky in assigning the Romanian principalities to the Byzantine Commonwealth.

KEITH HITCHINS  
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RICHARD GEORG PLASCHKA *et al.* *Innere Front: Militärassistenten, Widerstand und Umsturz in der Donaumonarchie, 1918*. Volume 1, *Zwischen Streik und Meuterei*; volume 2, *Umsturz*. (Veröffentlichungen des Österreichischen Ost- und Südosteuropa-Instituts, volume 8.) Vienna: Verlag für Geschichte und Politik. 1974. Pp. 420; 420.

This two-volume study focuses on the domestic situation and the decline of the internal order in

the Habsburg Monarchy in 1918. It deliberately disregards the impact of wartime diplomacy and of the military fortunes of war on domestic developments, though the importance of war and diplomacy is freely acknowledged (2: 334). The increasing disturbances and military insurrections in the Danubian monarchy in 1918 foreshadowed, of course, its ultimate dissolution. The three authors of the foregoing study are Richard G. Plaschka, head of the Institute for East European History and Research of Southeast Europe at the University of Vienna and two younger members of the institute who were his students. The work is based upon materials unearthed in the major archives and libraries of the succession states, in Laibach, Zagreb, Sarajevo, Budapest, Klausenburg, Prague, Brno, and Bratislava and in Vienna itself.

The primary task of the authors was the collection of materials which would shed light on the rapidly changing public opinion and all major forces operating in the disintegrating Austrian Empire—the growing opposition to the war-effort, the demand for national self-determination of the numerous non-German and non-Magyar peoples, and the diminishing loyalty of the army. Apparently endorsing Lenin's concept of the importance of the "struggle over the army" (1: 12 and 2: 332), the authors center their attention on the army's role in the struggle for the survival of the Danubian monarchy—a role the significance of which was clearly discerned both by national and social revolutionaries and by the defenders of the old order. The gathered materials would lead one to believe that inadequate food and clothing, resentment at army regulations, and hardships of the war rather than national grievances alone contributed powerfully to the final dissolution of the empire.

Students of the disintegration of the Danubian monarchy will be grateful for the mass of information which has been diligently collected by the authors. This reviewer regrets, however, that the authors refrained from drawing definite conclusions and from offering more elaborate interpretations; nor did they come to grips with past and current historiography on the breakdown of the Habsburg Monarchy. Important as the collected materials are, some tend to be repetitive. A well-known study such as A. J. May's *Passing of the Hapsburg Monarchy* is not listed in the bibliography. Though the scholarly equipment of the two volumes is on the whole impressive, the work is likely to disappoint the reader who was looking for more than a unique collection of source materials. Nevertheless, all interested in Austrian, East Central, and Southeast European history will be appreciative of the effort made. The books deserve to be widely used in the libraries of graduate schools.

ALFRED D. LOW  
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DAMIAN HUREZEANU, editor. *Nouvelles études d'histoire: Publiées à l'occasion du XIV<sup>e</sup> Congrès des Sciences Historiques: San Francisco, 1975*. Bucharest: Editura Academiei Republicii Socialiste România, Comité National des Historiens de la République Socialiste de Roumanie. 1975. Pp. 274. Lei 26.

There are a score of articles by twenty-one Romanian historians in this *Festschrift* for the Fourteenth International Congress of Historical Sciences meeting last year in San Francisco. Most articles focus on one aspect of Romanian history, chiefly in the modern period.

Authors include such well-known scholars as Constantin C. Giurescu, Ștefan Ștefănescu, D. Berciu, and Dan Berindei. Among others, Dan A. Lăzărescu has a suggestive but somewhat disjointed essay on the influences of Enlightenment rationalist thought in Transylvania and the contrasting *megale idea* of the Phanariots in Moldavia and Wallachia. Al. Zub, for his part, illustrates connections between some early Romanian historical and nationalist concepts.

About a third of the articles reflect archival research. This group includes two interwar-era studies by Eliza Campus and Gh. Zaharia respectively. With evidence from the Romanian Ministry of Foreign Affairs archives Campus offers the not-so-startling conclusion that the government sought to preserve the territorial status quo in league with both Little and Balkan Ententes. Zaharia adduces data from the Romanian War Ministry archives about the army's defensive posture, especially along the western frontier. He also contends that, despite newly built fortifications and arms factories, Romania was ill prepared at the end of the 1930s for modern warfare.

Several authors discuss Romanian contacts with America. For example, Paul Cernovodeanu examines the little some Romanians knew about the United States before 1830. Sergiu Columbeanu then describes the opening of Romanian-American commercial relations and considers an unrati-fied trade treaty, drafted in 1859, as the first major Romanian step toward independence. In discussing Romanian emigrants before 1918, Vasile Netea shows that they went mainly from Transylvania to the American Middle West, where they established churches, preserved national traditions, and sent financial and moral help to their former countrymen. A. Egyed reckons that 90 percent of Transylvanian emigrants from 1899 to 1913 were peasants, 50 percent were Romanians, and 90 percent went to America; he sees peasant emigration as resulting from rural overpopulation, poverty, and national oppression.

In all, this book contains some illuminating articles on various Romanian topics.

FREDERICK KELLOGG  
*University of Arizona*

V. M. KOLKER and I. Z. LEVIT. *Vneshniaia politika Rumynii i rumyno-sovetskie otnosheniia (Sentiabr' 1939-iiun' 1941)* [Romania's Foreign Policy and Romanian-Soviet Relations (September 1939-June 1941)]. (Akademiia Nauk Moldavskoi SSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 197.

The purpose of the authors of this study is to prove the validity of current Soviet assertions regarding the incontestability of Russian historic rights to Bessarabia. In brief, V. M. Kolker and I. Z. Levit assert that the incorporation of Bessarabia into the body politic of Romania at the end of World War I was a dastardly manifestation of bourgeois-land-lord imperialism. They further argue that the Romanian ruling clique of the interwar years conspired with Nazi Germany and anti-Communist forces of the West first to retain illegal possession of Bessarabia and, after June 1940, to recover Bessarabia by force of arms from its rightful Russian rulers.

The historic proof is standard focusing on in-vective, denial of the existence of a Romanian nationality in Bessarabia, and hailing the meritorious achievements of patriotic Moldavians and the wisdom of Soviet leaders committed to democracy and social justice. Two new historiographic features, however, are worthy of notice. The first is the justification of the Hitler-Stalin agreement of August 1939, as related to the secret clause regarding Bessarabia, as a reflection of legitimate Soviet concern for the historic rights of the inhabitants of that province. The second is the extensive utilization—albeit out of context—of references and citations from Western literature on Romanian and general international affairs as additional proof of the validity of Soviet contentions and, alternately, extensive denunciation of other Western writers whose "falsification" of the historic data is used as proof of the rightness of Soviet claims and actions.

Rational critique of the merits of the book appears impossible under the circumstances. If we were to accept Kolker's and Levit's axioms at face value there would be nothing to criticize. There are, however, serious questions regarding the validity of the authors' position that no amount of polemical dogmatism can ignore or refute. In fact, the only irrefutable contention is that the Soviet Union annexed Bessarabia in 1940. The significance of the book lies elsewhere: in the need for justification of that action in historic terms more than thirty years after the event.

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Boulder*

BÉLA K. KIRÁLY. *Ferenc Deák*. (Twayne's World Leaders Series.) Boston, Mass.: Twayne Publishers. 1975. Pp. 243. \$8.50.



Of the three Hungarians who loom large in nineteenth-century European history, only Ferenc Deák, the "Sage of the Fatherland," achieved political success. István Széchenyi, the aristocratic awakener of Magyar nationalism and liberal reform, committed suicide in an asylum near Vienna twelve years after the revolution of 1848, while Lajos Kossuth, the leader of the anti-Habsburg struggle for independence, left Hungary after the Austro-Russian military victory in 1849 to remain in exile for the rest of his long life. Deák, whose career was intertwined with those of Széchenyi and Kossuth, became the architect of the *Ausgleich* (Compromise) of 1867, his major claim to world leadership. It is indeed almost ironical to regard as a world leader this honest and most unpretentious Hungarian politician who never traveled west of Vienna and whose liberal ideology and knowledge of foreign countries were based solely on books. But insofar as the Compromise of 1867 may have saved, for good or bad, the Danubian monarchy in its dualistic form as a great power for another half century, Deák's contribution to European and world history is undeniable. Moreover, if decency were the only measure of political greatness, Deák's place among the foremost statesmen of the world would be secure.

Béla Király, the author of *Hungary in the Late Eighteenth Century* (*AHR*, 75 [1970]: 1157-58), shows convincingly in his new book how reluctant Deák often was in asserting his role as a leader of Hungarian liberalism ever since he first left an indelible mark on it at the Diet of 1832-36. Aside from modesty and awareness of his own limitations—virtues so rare in politicians—Deák possessed a talent for gauging the mood of his nation's decisive political force, the middle nobility and lesser gentry. Thus in the 1840s, when Széchenyi would have preferred Deák's moderate leadership to Kossuth's radicalism, Deák yielded to the latter both because he himself felt the need for revolutionary changes in Hungary's feudal legal system and also because most Hungarian liberals temperamentally sided with Kossuth. Yet in the two decades following the defeat of the revolution, Deák's cautious but unshakable talent for waiting until the right moment to assert constitutional rights proved a supreme source of strength. Deák also knew how to compromise in the interest of *Realpolitik*, as there was no alternative for Hungary at the time; to the exiled Kossuth, who reproached him for having abandoned their former shared ideals, Deák simply stated that the nation was with him.

It is one of the great merits of Király's well-balanced and lucid biography (the first in English since 1880 and the first by a Hungarian author since 1904) that it portrays Deák as a true representative of humanitarian liberalism: at the very beginning of his public life he raised his voice

against capital punishment in view of the social injustice inherent in it; at the end of his career he was voted down by his own liberal party because he thought that if there was state subsidy for a Magyar National Theater, then the Serbian minority also had a right to a subsidized theater of its own. The author's meticulous scholarship is indicated by his sophisticated analysis of the multiple domestic and international factors that led to the Compromise of 1867, although he could have stressed a little more in this context the initiatives taken by Francis Joseph. The number of errors—all but inescapable in a study under pressure to save space—is surprisingly small (for example, in 1831 Nicholas I rather than Alexander I was emperor of Russia; in 1846 the first steamship appeared on the Tisza River rather than the Danube, etc.). Furthermore, in spite of his consistent support of individual freedom, religious equality, civil rights, the supremacy of law over executive privilege, emancipation of the serf, and fairness toward non-Magyars within a Hungarian state, Deák was no radical social reformer as Király occasionally implies. Finally, the epithet "despotic" in reference to Vienna's Hungarian policy—supported by many a conservative and moderate Hungarian in the 1840s—appears overdrawn in view of Austria's limited power in Hungary. In balance, however, this is a most useful contribution to the growing American literature on East Central Europe.

GEORGE BARANY  
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*Études Historiques Hongroises 1975: publiées à l'occasion du XIV<sup>e</sup> Congrès International des Sciences Historiques par la Commission Nationale des Historiens Hongrois.* In two volumes. Budapest: Akadémiai Kiadó. 1975. Pp. 663; 639. \$80.00.

The Hungarian Academy has established the laudable practice of presenting the post-World War II International Congresses of the Historical Sciences with the research accomplishments of Hungarian historians in the five years between the congresses. Thus for the fifth time since 1945 scholars have the opportunity to survey recent Hungarian historical literature in two handsome volumes. I will not attempt the impossible task of analyzing all thirty-nine essays, which range from the early Middle Ages to post-World War II history and which cover Hungarian and general European history as well as questions of historical method and historiography in English, French, German, and Russian. A brief summary in a Western language follows the few essays printed in Russian, and a Russian abstract accompanies those in a Western language.

Diverse as the essays are in topic and quite naturally in quality as well, I will note some char-

acteristic features of the entire rich collection. I will also select some essays for specific brief comments, not on the grounds that they are necessarily superior to others but because they are of special interest to the Western and, in particular, the American reader.

A strong and welcome feature of the articles as a whole is the emphasis on historiography. This pertains notably to the studies by Emil Niederhauser on Eastern Europe in recent Hungarian historiography and the more controversial one by György Mérei on bourgeois historiography in the Federal Republic of Germany. The reader, however (and this is a welcome new trait), will find abundant references to literature in Western languages that in a substantial number of cases are objectively abstracted and interpreted. Of great, indeed outstanding, value is the amazingly rich, annotated, selected bibliography on Hungarian historical literature published between 1969 and 1973 at the end of volume two. With particular regard to annotation I must regretfully note that to my knowledge no similar enterprise has recently been undertaken in Western languages on a regular basis.

One should note that the editors have placed their chief emphasis in the selection of topics, far less than in the past, on socioeconomic history, particularly in regard to ancient and medieval times. Of a very different character—to take just two distinguished examples—are the essays by György Györffy on the seminomadic state in early medieval Hungarian history and that of Jenő Szűcs on the chronicles of Simon of Kéza in the thirteenth century. Zsigmond Pach's essay on Hungary and the Levantine trade in the Middle Ages is remarkably open-minded.

The majority of articles deal, of course, with modern history, balancing well the eras of the Reformation and Counter Reformation, the Enlightenment, the nineteenth and twentieth centuries, and contemporary history. Kálmán Benda offers a solid survey on the conflict between sixteenth-century Habsburg absolutism and estate rights and demands. Agnes Varkónyi discusses in a more original fashion the anti-Turkish coalition in Leopold I's first war against the Ottoman Empire. Eva Balász's able account of the relations to Hungary of Count Karl Zinzendorf—the high bureaucrat and famous traveler and diarist—deserves attention since Balász is one of the very few who have shown interest in the activities of this most remarkable person. Peter Hanák makes a new and certainly arguable contribution to a somewhat overworked theme, the preludes to the Austro-Hungarian Compromise. According to him the dualistic trend rather than the semi-federalist shell characterizes the October Diploma of 1860.

Two notable essays deal with the question of party origin and development in the Austro-Hungarian labor movement on an empire-wide basis. Yet neither the one by Edit Siklós-Vincze on the era from 1868 to 1872 nor the more one-sided article by Tibor Erény on the situation in the late nineteenth century does full justice to these interesting topics. Sándor Balogh's article on the issue of democracy in Hungarian political parties after World War II should generate controversy. It is obvious that all three authors had to struggle with limitations of space.

Of particular interest for the American reader are the essays by György Szabad and Julianna Puskás. Others have previously discussed Szabad's theme, "Kossuth on the Political System of the United States of America," but to my knowledge not in as interesting a manner. Puskás's statistical analysis of Hungarian immigration to the United States from the 1850s to 1914 helps explain considerable variations in the immigration curve.

Finally, I should call attention to the introductory essay by Lajos Elekes on historicism, ahistorism, and antihistorism in contemporary bourgeois historical literature. It is historiographical in form but philosophical in content, and it is based on a wide knowledge of pertinent literature in Western languages. In his final analysis the author naturally compares favorably what to him are the certainties of historical materialism with the probing and steadily shifting courses of Western philosophy of history. To argue the point seems futile, but one should note that Elekes lucidly, and on the whole objectively, presents several lines of thought that he does not share.

All things considered, these two new volumes published by the Hungarian Academy are a distinct contribution to our discipline.

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SALO WITTMAYER BARON. *A Social and Religious History of the Jews: Late Middle Ages and Era of European Expansion*. Volume 16, *Poland-Lithuania, 1500-1650*. 2d. rev. ed. New York: Columbia University Press. 1976. Pp. 460. \$17.50.

Scholars generally agree that the sixteenth century was Poland's "golden age" of cultural, political, and economic development, an era marked in 1569 by the constitutional merger with Lithuania. This event, the Union of Lublin, created a heterogeneous "federation" of Poles, Lithuanians, Ukrainians, Belorussians, Jews, Germans, Armenians, and other peoples, with an accompanying melange of languages, creeds, and judicially recog-

nized legal codes. The Jews were one of the most important minorities in numbers and, particularly in the urban centers of trade and commerce, economic power. Their social and economic rise—and subsequent decline—paralleled to a surprising extent the rise and decline of the Polish-Lithuanian Commonwealth as a whole, the Cossack insurrection of 1648 striking a blow from which the Commonwealth, and its Jewish population, never fully recovered.

This significant chapter in the history of world Jewry is the subject of Salo Wittmayer Baron's book. As was true of the preceding volumes of his mammoth history of the Jews (and it should be noted in passing that, the title notwithstanding, they contain much more than "merely" social and religious topics), this book is a monument of impeccable scholarship. The author reveals a deep knowledge of the historical period in question, and of the complicated political, social, and economic fabric of the Commonwealth. Baron's familiarity with the relevant literature, mostly in Polish, is evident in the chapter notes (comprising a third of the book) and lead the reader to myriad sources. In addition to providing excellent documentation, he tells the story in a good style; to quote from Baron's opening paragraph: "While the lamps were going out in the Jewish quarters all over Western and large parts of Central Europe, new lights were being kindled in the less developed valleys of the Vistula, Dniester, Dnieper, and Niemen . . ." (p. 3).

The book consists of four chapters. The first, "Poland's Golden Age," discusses political, social, and intellectual developments to the time of King Stephen Bathory's death in 1586, an era when the Renaissance and, later, the Reformation came to flower in the Commonwealth. The two giants of Polish rabbinic learning then were Moses Isserles and Solomon Luria, at a time when "rabbinic literature received fresh stimuli and reached new heights of achievement in Poland, heights which were not again attained for generations thereafter" (p. 32). The second chapter, "Poland-Lithuania on a High Plateau," continues the account to 1650, during which time the Commonwealth was governed by the Vasas and the religious freedoms of the Reformation began to be eroded by an increasingly intolerant Counter Reformation. Nonetheless, the Jews were able to hold and consolidate their earlier gains. As the author points out in his third chapter, "Territorial and Numerical Expansion," in the 1586–1650 period "the rapid development and expansion of the settlement area, the great adjustments to a changing economy, and the vast progress in population size and popular education under Sigismund III and Wladyslaw IV made that sixty-year span one of the most fertile

periods in the history of East-European Jewry" (p. 164). The final chapter, "Socioeconomic Restructuring," describes in detail how the population growth during the one and one-half centuries before the Cossack uprising of 1648 gave rise to a revolutionary transformation in the general social and economic structure of Polish Jewry, stimulated by opportunities in industries, professions, and certain branches of agriculture.

The book, in short, is the best currently available on the subject.

HARRY E. DEMBKOWSKI  
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MARIAN KALLAS. *Organy administracji terytorialnej w Księstwie Warszawskim* [Organs of Territorial Administration in the Duchy of Warsaw]. (Uniwersytet Mikołaja Kopernika, Rozprawy.) Summary in French. Toruń: Uniwersytet Mikołaja Kopernika. 1975. Pp. vii, 237. Zł 30.

Historians accept without reservation the thesis that Napoleon replaced Poland's ancient, chaotic, decentralized administration with a thoroughly modern, orderly, and centralized system. They have also generally believed that the new Polish administration was an exact replica of what Napoleon had introduced in France; it is this latter point of view that Marian Kallas has challenged.

This challenge is not so much a repudiation as it is a modification of the traditional view. In the course of his research the author visited libraries and archives of more than a dozen cities to examine hitherto unexploited manuscript materials. These documents revealed to him a local administration (in the departments, districts, towns, and villages) somewhat at variance with the French model, especially in its structure and in its actual operation. Kallas found that Prussian patterns and regulations as well as Polish customs and institutions persisted and thus limited the total acceptance of the French model.

While Kallas' study contributes to our understanding of the Polish administrative system, his analysis does not differ greatly from that of past historians. The latter have dealt more with the central or national organs of administration and have based their description on the provisions of the Duchy's constitution and relevant legislation. Kallas, on the other hand, deals with the local or territorial administration and its actual functioning. It is at that lowest level of administration that he finds the greatest deviation from the French pattern.

Some unusual features of this book deserve to be mentioned. The bibliography lists only the manuscript materials used by the author and their location in the various local libraries and archives of

Poland. The printed documents and secondary works are included in the footnotes which comprise half as many pages as the text itself. Western scholars will find the French summary very useful.

CHARLES MORLEY  
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CELINA BOBIŃSKA and ANDRZEJ PILCH, editors. *Employment-Seeking Emigrations of the Poles World-Wide: XIX and XX C.* Translated by DANUTA E. ŻUKOWSKA. (Zeszyty naukowe Uniwersytetu Jagiellońskiego, number 417; Prace polonijne, number 1.) Cracow: Nakładem Uniwersytetu Jagiellońskiego. 1975. Pp. 194. Zł 30.

This collection of essays relating to the Polish emigration movements provides an introduction to present-day Polish historiography on the subject. Celina Bobińska makes in the introduction a thought-provoking apologia for the non-statistical, non-quantitative approach of recent Polish emigration studies, arguing that the complex nature of Polish emigration, the division of the Polish people among three different governments keeping different types of records, the loss during World War II of many extant emigration records, and the undesirability of converting Polish workers and peasants into numbers or mere cogs in the world labor market have prevented Polish historians from undertaking the types of quantitative studies which are becoming so popular in the West. One anticipates, then, that the remaining essays will be microstudies—of individuals, of villages, or of regions for which the proper, homogeneous records do exist and about which some microconclusions might be made.

Unfortunately, only Andrzej Pilch's "Migrations of the Galician Populace at the Turn of the Nineteenth and Twentieth Centuries" comes close to this mark. The other essays resort to basing broad ideological generalizations on scattered and carefully chosen statistics. All of the essays repeat the process of asking what constituted emigration as opposed to temporary migration, which territories were Polish and therefore eligible for inclusion in the study, and which percentage of those leaving a given region was Polish and which was of a minority nationality. The authors, however, reach no conclusions, either singularly or collectively. This repetitious process nonetheless assures recognition and discussion of the special problems of the non-Polish nationalities, of the Jews, and of women in the labor market.

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JÓZEF GARLIŃSKI. *Fighting Auschwitz: The Resistance Movement in the Concentration Camp.* London: Julian Friedmann; distributed by Holmes and Meier, New York. 1975. Pp. xi, 327. \$12.50.

Because the human mind has come to equate Auschwitz with the extermination of European Jewry, one might expect this book to deal with the Jewish Holocaust. Not so. The gas chambers and crematoriums stand only at the edges of this work. Instead, Józef Garliński has contributed a valuable study of a little-known underground, made up mostly of non-Jewish Poles, which was built among the longer-term inmates of the Auschwitz concentration camp. Since most Jews deported to Auschwitz were quickly murdered, throughout the camp's existence Polish political prisoners comprised the largest part of its population.

Central to the book is Witold Pilecki, a leader of the outlawed Polish army who let the Nazis capture him in order to get into Auschwitz to initiate a military underground there. Pilecki organized Poles of widely different political ideologies into a clandestine network called the Union of Military Organization (ZOW, after its Polish initials). By infiltrating influential camp offices, ZOW leaders succeeded in ameliorating life for many inmates. In addition, men placed on details that operated near or outside camp boundaries made contact with the local underground and opened channels that carried reports on camp conditions to the Warsaw headquarters of the illegal Polish Home Army and thence to the Polish government in London. For a brief time, Pilecki even sent radio messages from a transmitter hidden in Auschwitz. This work was done under constant risk of discovery, torture, and death. Many of the camp underground paid this price, including twenty-eight ZOW leaders who were killed in an October 1943 bloodbath.

Despite its achievements, the ZOW never attempted Pilecki's main objective: an armed revolt to liberate the camp. Pilecki envisaged a coordinated assault on Auschwitz by partisans from the outside and the ZOW from within. But ZOW messages to Home Army headquarters in Warsaw concerning attack plans received no answers. So in April 1943 Pilecki escaped from Auschwitz and fled to Warsaw to plead personally for the revolt. He was unable to budge Home Army authorities from the conclusion that their forces and arms were too limited and the ZOW too weak to risk battle with the SS garrison at Auschwitz, backed by German Army units in the vicinity. The plan was deemed practical only in the event of a countrywide rising. Conditions for the revolt never developed. In Garliński's words, "There never was any fight to liberate the camp. The countrywide uprising against



the Germans never took place. . . . Help from the West [to the Home Army] proved too small even to save Warsaw" (p. 256). In 1944 the Russian Army, within striking distance of Auschwitz, showed no interest in a joint effort to free the camp. Ironically, the few uprisings which did occur in Auschwitz were not planned by the ZOW, but were desperation actions by doomed Jews. The single *armed* revolt took place when Jews in a body-disposal detail, facing death themselves, blew up one crematorium and killed and injured several guards.

Garliński proves the existence of an effective underground in Auschwitz, and he demonstrates that, contrary to some postwar claims, Communists neither created it nor were importantly involved in it. The author also makes understandable the Polish underground's failure to attempt to liberate Auschwitz. But one is left wondering why neither the ZOW nor the outside Polish resistance considered sabotage of the gas chambers and crematoriums. Nor is it clear why no comprehensive description of the Jewish situation at Auschwitz reached the West until two Jews escaped and exposed the full facts in April 1944. What became of the earlier reports on this which the ZOW must have forwarded to Warsaw?

Garliński's writing is clear and engaging. The sources look solid, though the study necessarily rests heavily on Pilecki's own unpublished account (written in 1945) and on statements given the author some twenty-five years after the events by survivors of Auschwitz. Too many substantial paragraphs lack footnotes. Finally, in view of Garliński's finding that "there never was any fighting" by the ZOW (p. 265), the book's title seems inappropriate.

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ARVIDS ZIEDONIS, JR. *et al.*, edited with an introduction by. *Baltic History*. (Publications of the Association for the Advancement of Baltic Studies, number 5.) Columbus, Ohio: Association for the Advancement of Baltic Studies. 1974. Pp. iv, 341. Cloth \$16.00, paper \$8.95.

These twenty-eight essays are a selection from the papers presented at the Third Conference on Baltic Studies at the University of Toronto in May 1972. Using a chronological pattern, the editors have arranged these essays, which are widely disparate in themes and scholarship, under four headings: the later Middle Ages, the eighteenth and nineteenth centuries, the period between the World Wars, and World War II and its aftermath.

Among the medieval essays, Thomas Noonan challenges the theory of Russian domination over the Estonian Chud', expressed through the collection of tribute and the conscription of troops, and convincingly establishes Estonia's independence during the period 850-1015. Romuald Misiunas concludes that Sweden never was a real threat in the post-Nystad period, and this "probably" was apparent to the Russians, who used the Treaty of Nystad to ensure Sweden's internal weakness. The same treaty guaranteed the Baltic Germans' privileges, and so domestic and international problems in the Baltic were interrelated.

Three essays deal with modernization during the period 1860-1914. In Toivo Raun's Estonian example, the impulse toward modernization was external, and the Estonians did not lead the policy-making process. The Russification of the Baltic Germans, as described by Michael Haltzel, was "a dysfunctional aspect of imperial modernization"; the religious, judicial, and educational Russification efforts were modernizing policies carried to counterproductive extremes. Finally, Andrejs Plakans delineates (with ample statistics) the role of a Latvian-speaking urban elite in the creation of a Latvian culture.

Bruno Kalniņš, in an essay that is part memoir, recounts the role of the Workers' Council in Riga as the center of power from March to September 1917. Kalniņš was a Menshevik member of the council's executive committee and presidium, and the editor of its newspaper. According to Kalniņš, from the beginning the council supported the Latvian national movement for autonomy and then independence. Regarding the Estonian movement for independence, 1917-18, Arved von Taube pinpoints the sympathetic supporters in Germany's *Auswärtiges Amt*, such as Count Ulrich von Brockdorff-Rantzau, who opposed the German policy of Baltic annexations. Their motivations stemmed mainly, however, from the exigencies of Russo-German relations. Utilizing the Hoover Institution's archives from the Latvian Legation in Stockholm, Edgar Anderson details the inexorable developments in the Soviet Union's mutual assistance pacts with the Baltic States, 1939-40.

This collection of essays is significant as a picture of current research on Baltic history by American and foreign scholars. Predominant are traditional methodological approaches to political questions and only the introductory essay by the late Oswald Backus, "The Impact of the Baltic and Finnic Peoples upon Russian History," suggests provocative possibilities for comparative studies or new social and intellectual topics.

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HUGH I. RODGERS. *Search for Security: A Study in Baltic Diplomacy, 1920-1934*. Hamden, Conn.: Archon Books. 1975. Pp. xi, 181. \$12.00.

Hugh Rodgers explores Latvian efforts to create a feasible Baltic security system, with particular emphasis on the work of Zigfrids Meierovics, the first foreign minister of Latvia. Meierovics, as did his successors, pursued a number of policies that at one time or another favored ties with Poland, Germany, Soviet Russia, and the major Western powers. Unfortunately, German-Soviet opposition to Latvian links with Poland, as well as general Western indifference to Baltic security needs, effectively ended, according to Rodgers, prospects for a general Baltic alliance system.

On the other hand, Berlin also proved uninterested in a close security arrangement with Riga, while the conclusion of a Latvian-Soviet trade agreement and nonaggression pact in 1927 received widespread domestic and international opposition. This response, Rodgers concludes, left Latvia and the Baltic states with no more security options and sealed their fate thirteen years before their absorption into the USSR.

Between 1930 and 1934, Latvia drifted from scheme to scheme to find some security agreement to counter the growing German threat. When all other plans proved futile, Riga grudgingly accepted, with Soviet approval, the empty prospect of a Baltic state alliance system—the Baltic Entente.

Overall, Rodgers' study provides valuable insight into the domestic and international considerations that affected Latvian foreign policy during the period 1920-1934. Unfortunately, his failure to use the important British Foreign Office records in London with their invaluable, lengthy notations, and his dependence on the National Archives' incomplete collection of German Foreign Office files raise questions regarding the depth of his research, particularly in light of his introductory statement that the "opening of the Baltic republics' archives in the future could alter this study only in minor detail." Despite these failings, his book provides scholars with an interesting view of foreign-policy decision-making by small powers during the interwar period.

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R. G. SKRYNNIKOV. *Ivan Groznyi* [Ivan the Terrible]. (Akademiia Nauk SSSR, Seriiia "Istoriia nashei Rodiny.") Moscow: Izdatel'stvo "Nauka." 1975. Pp. 246. 87 k.

Historians of medieval Russia have long debated the significance of the reign of Ivan the Terrible. This relatively brief political history of Ivan's reign

by one of the Soviet Union's most distinguished experts, R. G. Skrynnikov, rests upon his earlier, more detailed monographic studies—most notably *Nachalo oprichniny* (1966) and *Oprichnyi terror* (1969).

Skrynnikov's thesis—that the dominant theme of Ivan's reign was his attempt to create an unlimited autocracy—is hardly new. What is controversial is his assessment of that attempt. While accepting the view that Ivan was striving to eliminate opposition to centralized government by destroying the power of the aristocracy, Skrynnikov contends that Ivan's targets changed with time and circumstances. In the 1550s the tsar backed the pro-*dvoriane* (middle service class) reforms to undermine the boyar aristocracy. Then, in the 1560s, he turned increasingly to open attacks upon other elements in the Muscovite elite. The major repressions of 1565 were directed specifically against the princely aristocracy. In the late 1560s his campaign against "opposition" broadened into a reign of terror, whose targets included not only the old boyar families, but the central bureaucracy, the Church, and Novgorod. Whereas other historians—such as A. A. Zimin—have looked upon the Church and the city of Novgorod, for example, as bulwarks of resistance to centralization, Skrynnikov argues that they had been supporters of the monarchy. Thus, Skrynnikov concludes, the terror was politically senseless because the result was to alienate formerly loyal elements.

The political instability, the worsening economic crisis, and military defeats forced Ivan to abandon massive terror, but suspicion, distrust, and sporadic repressive measures continued. In the last years of his life, Ivan made his peace with the boyars to assure the succession of his feeble-minded son, Fedor. Thus, at his death, the political structure of the regime remained unchanged, with the Boyar Duma still the highest organ of government. But despite this surface continuity, Skrynnikov finds that Ivan succeeded in breaking the former power of the aristocracy and thereby laid the basis for the reassertion of a strong centralized regime under Boris Godunov.

Not all historians of the period will accept Skrynnikov's assessment of Ivan's aims, policies, and achievements, or his evaluation of source materials. His provocative analysis, based on extensive research in primary sources, deserves, however, the respectful attention of all students of medieval Russia.

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S. O. SHMIDT. *Stanovlenie rossiiskogo samoderzhavstva: Issledovanie sotsial'no-politicheskoi istorii vremeni Ivana Groznogo* [The Establishment of the Russian Au-

tocracy: A Study of the Sociopolitical History of the Time of Ivan the Terrible]. Moscow: Izdatel'stvo "Mysl'." 1973. Pp. 357.

The three long essays brought together here represent the results of a quarter century of research and writing by a prominent Soviet historian, who has concentrated his considerable labors quite consistently upon the "sociopolitical" history of mid-sixteenth-century Muscovy. Although most of what is included in this collection has been published in a similar form elsewhere, its appearance in a single volume provides the occasion for a general appreciation of Shmidt's findings and method of investigation.

The first essay, "The Beginning of the Tsardom of Muscovy," is in fact devoted to an analysis of the palace coup and the associated mob action that Shmidt calls the "uprising" (*vosstanie*) of June 1547; the second considers at some length problems of the *zemskie sobory* (the quotation marks of Shmidt's chapter heading, proper for such an anachronism, do not appear consistently in the text); the third is an attempt to reframe the historiographically venerable, but still mysterious, question of the Muscovite *mestnichestvo* system.

Although the nature of the three essays is thus varied, each contributes to a general impression of conceptual and definitional confusion and contradiction. In each of them, for example, the author devotes laudably extensive attention to the study of sources, and he often repeats healthy cautions about the methodology of analysis of pre-modern, especially narrative, materials. But he is able at the same time to hold that "class analysis is, of course, indispensable before all else in attempts to determine the historical value of a source" (p. 9), a statement that is both methodologically retrogressive and rather unexpected in the work of a historian writing in a classless society and about a society in which class is at best an anachronistic concept.

Elsewhere (particularly in the essay on *mestnichestvo*) Shmidt makes perceptive observations about the stultifying effect upon historians of "historiographic legends," but he himself frequently falls into the thrall of precisely such legends, in particular those originated by Russian public figures of the nineteenth century.

Finally, although he seems to be aware of certain contemporary Western approaches to the study of politics in traditional or premodern societies (he cites both such Western authors as Braudel and Huizinga and the innovative Soviet works of A. Ia. Gurevich and Iu. M. Lotman), for the most part Shmidt treats his material quite conventionally without making the attempt, upon which such contemporary approaches are based, to place him-

self within the perceptual and institutional system of Muscovy and to determine its "deep structures." As a result his conclusions are not only largely speculative, but they also tend to create the false impression that sixteenth-century groups—boyars, "bourgeois," and mob—were acting within a political culture essentially indistinguishable from more familiar modern ones, especially that of pre-Revolutionary Russia. For example, he seems to assume that a Muscovite mob might in 1547 actually think about seizing political power—an utterly anachronistic notion—and he applies to the events V. I. Lenin's concept of a "revolutionary situation," which has no place in a discussion of sixteenth-century politics.

Thus Shmidt appears to be operating in a contradictory conceptual universe regulated, on the one hand, by the modern scholarly ideas he professes to accept and, on the other, by "class analysis" and a peculiar reliance upon nineteenth-century "progressive" ideas. As a result, his work ends having begged crucial questions about the central features, the deep structures, of Muscovite politics: What was political power in this culture? Who wielded it—clans, "parties," individuals, "the state"? What were the rules of the game, and how did participants perceive it?

Although Shmidt makes several astute observations about the complex source materials from this period and elucidates numerous factual details, he does not provide a clear understanding of how Muscovite politics worked, and he therefore is unable to provide a convincing account of either the course of the events described or their significance.

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CH. E. KARDANOV. *Iz istorii otnoshenii mezhdu adygskimi narodami i Rossiei v XVI veke (1550–1580)* [From the History of the Relations between the Adigei Peoples and Russia in the Sixteenth Century (1550–1580)]. Nal'chik: Izdatel'stvo "El'brus." 1972. Pp. 164. 79 k.

This book is an attempt by a Soviet historian, himself an Adigei, to treat the establishment of relations between the Muscovite state and the Adigei peoples, the Cherkess and the Kabardinians of the north Caucasus, from 1550 to the end of the sixteenth century. The author bases the work almost entirely on Russian sources, since little or nothing in the indigenous languages has survived, although he attempts to supplement the historical record through the use of Adigei folklore, folk songs, and oral tradition. Kardanov's major theses are that the Adigei peoples voluntarily sought the overlordship and protection of the Mus-

covite Russians from the middle of the sixteenth century onward; that this policy was dictated and justified by their common cause against dangerous Ottoman expansionism; that the centralization and expansion of the Great Russian state at Moscow was sanctioned by the objective laws of history; and that Russia's expansionary role in the north Caucasus was necessarily a progressive one.

The work tends to perpetuate a number of historical clichés that inhibit solid research in the area. The most serious of these is Kardanov's insistence on the image of traditional Ottoman hostility and aggression toward Moscow. A proper study of the historical records available should show that Ottoman-Muscovite relations were quite good until the late 1560s, that the Ottoman state was in fact one of Moscow's largest trading partners, and that their relations began to show serious signs of deterioration only after Moscow's seizure of Kazan, Astrakhan, and the Volga route in the 1550s and Muscovite expansion into the north Caucasus in the 1560s. Second, Kardanov's portrayal of Moscow faced by a hostile phalanx of Muslim and Turkic powers dominated and directed by Istanbul is exaggerated, since the Kazan Tatars, the Nogai federations, and smaller groups of the north Caucasus constantly formed and dissolved alliances irrespective of ethnic or religious allegiances. Moscow was as often the beneficiary as the victim of such shifts. Third, Kardanov interprets the Ottoman-Crimean campaign of 1569 against Astrakhan as a direct strike against Muscovy when in fact it was directed against Safavid Persia and had been provoked by Moscow's seizure of Astrakhan in 1556 and construction of the Terek fortress in the north Caucasus in the 1560s, steps that jeopardized Istanbul's lines of communication to the East.

The monograph is useful in that it attempts to use Adigei sources (especially concerning the circumstances of the execution of Mikhail Cherkasskii in 1571) and highlights the prominent role played by the Caucasian peoples in the Muscovite state. Ivan IV was married to the daughter of the Kabardinian prince Temriuk Idarov, and his elite *oprichnina* guard was commanded by Temriuk's son Saltankul who had taken the Russian name of Mikhail Cherkasskii. There is an interesting and important story to be told here. One hopes that Kardanov will pursue it more carefully in the future.

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EDGAR C. DUIN. *Lutheranism under the Tsars and the Soviets*. In two volumes. Ann Arbor: Xerox University Microfilms. 1975. Pp. xiv, 640; viii, 285. \$43.00.

This first general history of Lutherans in Russia is an unedited typescript based on memoirs, newspapers, and other published materials. Edgar C. Duin's study, which is more narrative than analytical, traces with adequate detail Lutheran history from the earliest sixteenth-century Livonian congregations until today. He describes all Lutheran ethnic groups and all facets of Lutheran life from settlement, social and economic development, parochial education, and church-state relations to the virtual cessation of organized Lutheran worship under Soviet rule.

The author structures his text around the emergence of a Lutheran "national church" embodied in a Lutheran General Consistory created by the 1832 Church Ordinance, which brought together all tsarist Russia's disparate Lutheran elements: Baltic Lutherans, German colonists, and congregations in large Russian cities. Duin's focus on a national church, however, implies a sharing of basic concepts that never existed. The Baltic church was confessional and had a history of subordination to local government. Conversely, many colonists combined a pietistic emphasis on spirit over doctrine with a search for freedom from secular interference in religion. Nineteenth-century urban congregations maintained close ties with Germany and consequently reflected anticonfessional attitudes in vogue there. Contact among these groups after 1832 remained superficial because of cultural and religious as well as sheer physical distance. The strongly confessional ordinance had little religious meaning outside the Baltic provinces; even there its statism violated traditions of local autonomy. An ordinance alone could not call into being a national church, even though it created a bureaucratic apparatus for the whole empire. The consistory was dissolved in 1917; hope for an alternative Lutheran Synod died in the 1930s.

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S. M. TROITSKII. *Russkii absoliutizm i dvorianstvo v XVIII v.: Formirovanie biurokratii* [Russian Absolutism and the Nobility in the 18th Century: The Formation of a Bureaucracy]. (Akademiiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1974. Pp. 394. 2 r. 02 k.

Prior to his untimely death earlier this year, Sergei Martinovich Troitskii assembled the above volume from several of his previously published studies and some newer material. While the resulting monograph varies somewhat in quality, there can be no denying its significance. It provides a wealth of information crucial to an understanding of the

development of Russian government and society in the eighteenth century.

The author's task is to explain how the administration that arose during and after the Petrine reform became a sturdy prop of absolutist rule. He views political power as having a social impact that is not often encountered in Soviet historiography. By redefining nobiliary status in the 1722 Table of Ranks the tsar-reformer did not dilute the position of a hereditary estate, but laid the foundation for the rise of a new social layer (*proshloika*). In Troitskii's estimation, this offspring reflected the dominant traits of a traditional service caste, but was also invigorated by recruiting talent from lower strata. His work is largely an effort to determine how and at what point the eventual balance between these two elements was struck.

The author's close study of the Table's origins demonstrates that Peter I intended no reduction in the position of the nobility, but sought to upgrade Russia's civilian administration by making it nearly as prestigious as her military. Besides the formal education now required of them, younger nobles would gain experience in statecraft by gradually ascending the hierarchy of posts. Although the author at times makes Petrine policy appear too rational, he does emphasize the fact that the state intended primarily to recruit nobles for governmental positions. Only within these circumscribed limits would service replace birth as a status determinant. Troitskii concurs with previous accounts of how little the members of Russia's most prestigious social group cared for civilian duties, but goes on to detail the consequences. In the absence of close supervision by St. Petersburg, provincial administrators circumvented decrees by appointing commoners to posts that were included in the Table of Ranks—effectively bringing the accolade of nobleman within their grasp.

Any question as to the eventual results of this process can be answered by turning to the extensive fourth chapter, comprising nearly half of the book. The information that is summarized here in tabular form is derived from a thorough examination of the Russian administration at the middle of the century. While the fundamental source is a census of over 5000 civilian officials, Troitskii has vastly expanded its significance by consulting a myriad archival and published records. One most impressive example of this painstaking approach is the author's ability to complete our knowledge of some twelve hundred officials in the original contingent who "neglected" to indicate their social origins. In the process of classifying these men as nobles or commoners by such traits as their education, official duties, or family ties, he provides a fascinating tableau of the mores

of this period. The resulting data not only show that a large proportion of officials were of non-noble parentage (some 78 percent), but how this figure varied within the Russian administration.

Beyond its aggregate treatment of social change, this monograph enables us to appreciate the factors that attracted an individual to the service and influenced his ability to ascend its echelons. The 630 families that are traced through three generations of service show how personal connections and skills (knowledge of languages, for example) accelerated progress in the hierarchy. The author also emphasizes the incentive of salary, underscoring his point by frequent mention of the number of serfs a servitor owned. For many noblemen—officials who relied on their careers for income the estate seems to have had largely symbolic importance.

The difficulty of summarizing a work of this scope should give some indication of Troitskii's outstanding scholarship. His death is surely a loss to the profession.

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MICHAEL F. HAMM, editor. *The City in Russian History*. Lexington: University Press of Kentucky. 1976. Pp. 349. \$15.00.

GILBERT ROZMAN. *Urban Networks in Russia, 1750-1800, and Premodern Periodization*. Princeton: Princeton University Press. 1976. Pp. x, 337. \$16.50.

As William Blackwell correctly points out in his conclusion to the volume edited by Michael Hamm, "the study of Russian urbanization is hardly under way." It is in this context that these two very different works are welcome and impressive steps forward.

The sixteen essays in the Hamm volume, written mainly by historians and geographers, form an eclectic collection covering the whole of Russian urban history. The unifying theme is an old one, but a good one: the "critical role of the state in shaping the nature of the Russian city . . . in all periods." Thus Lawrence Langer concludes that princely domination of medieval Russian towns prevented the development of urban autonomy and self-government. In good surveys on the sixteenth and seventeenth centuries David Miller and Michael Hittle also stress the subordination of the city to a tax-hungry state. The rise and decline of urban autonomy is the focus for the nineteenth century. Frederick Skinner has a fine study on planning in Odessa, while Michael Hamm argues that a breakdown in urban services due to the lack of municipal fiscal autonomy was an important cause of revolution in 1917. The geographers con-



centrate on the relationship between industrialization and urbanization, and reach somewhat contradictory conclusions. Roger Thiede argues convincingly that industrial growth was a prime factor in urban growth in New Russia before 1914. In contrast, Robert Lewis and Richard Rowland, using regression analysis and a national framework, conclude that the growth of the service sector was a more important factor in pre-Stalinist urbanization. Frederick Starr's discussion of urban planning before the Revolution and the subsequent triumph of the old planners in the 1920s is fascinating. William Blackwell's conclusion successfully coordinates the essays around the role of the state, modernization, and social welfare. Although this collection makes no claims for great intellectual originality, it does provide a valuable introduction to a neglected field.

Gilbert Rozman's *Urban Networks in Russia* is a more ambitious and a more difficult work. The monograph—a sequel to Rozman's earlier study, *Urban Networks in Ch'ing China and Tokugawa Japan* (1973)—has an innovative methodology and an original thesis. Beginning with the central-place theory of geographers, Rozman elaborates his own model of premodern urban networks. An urban network is “a hierarchy of settlements differentiated according to population or commercial and administrative functions”; Rozman distinguishes seven levels, from the marketing settlement to the national administrative center. He adds to this typology a stage theory of urban network progression, in which the increase in types of settlements produces seven stages of premodern urban development. With his formal model, Rozman carefully studies Russian cities from the earliest times to 1800. He then compares Russian developments with those in premodern China and Japan, as well as those in France and England. The author concludes that Russia's premodern growth has been seriously underestimated and that Russia was “an unusually dynamic premodern country.” He urges us to rid ourselves of “the myth of Russian backwardness” and classify Russia “among the advanced premodern societies in urban and most likely other measures of development.” Rozman further concludes that the urban networks approach constitutes a breakthrough in premodern periodization, and hence understanding.

It is not easy to pass judgment on the value of Rozman's work; the final verdict will certainly require the opinion of specialists from the many fields he touches. Nonetheless, I find the treatment of Russia thorough, boldly imaginative, and generally convincing. Rozman's attack on the idea of Russian backwardness, which has sometimes threatened to become a debilitating crutch, is provocative and desirable. I do have reservations

about this linear and (apparently) universal stage theory, and wonder how much has really been explained. If all five countries studied had reached the seventh and highest stage of premodern urban development by 1700, as Rozman contends, then how is one to account for the undeniable differences which remained? References to as yet unspecified sub-stages within the seventh and highest stage seem to indicate that Rozman himself is aware that there is more to be explained. Be that as it may, this is an exciting and highly innovative accomplishment, which deserves praise and serious consideration.

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ALEXANDER VUCINICH. *Social Thought in Tsarist Russia: The Quest for a General Science of Society, 1861–1917*. Chicago: University of Chicago Press. 1976. Pp. ix, 294. \$15.50.

Alexander Vucinich discusses theories about society which some Russian intellectuals subscribed to during the period 1861–1917. Many of his discussions are interesting and informative and are based on a useful, wide-ranging bibliography. They represent, however, only some theories of society, not a thoroughgoing survey of social thought in Russia.

The apparent basis for the author's selection is his determination to tie prerevolutionary social theory to radical ideologies. About two-thirds of the substantive part of the book are devoted to the essentially ideological writings of some radical, nonacademic intellectuals; somewhat less than a third is given to academic sociologists—that is, men whose concern for science overshadowed their ideological commitments. Not surprisingly, one of the book's major contributions is to illuminate the contrasts between Populist and Marxist views of society and history. Of special interest to the historian of radical ideology is a chapter on the quasi-Bolshevik, A. A. Bogdanov.

Vucinich repeatedly asserts that journalists like Chernyshevskii and Pisarev and free-lance philosophers like Lavrov and Mikhailovskii were a fountainhead, of sorts, from which all subsequent Russian social thought flowed. Judging from his own account, however, the academic social theorists owed very little to these would-be intellectual forebears. One gets the impression that academic social scientists in Russia drew their inspiration from other scholars, primarily Western Europeans. Consider B. A. Kistiakovskii, for example. He took the trouble to criticize Mikhailovskii (who was by far the best scholar among the ideologues), and he



represents, therefore, Vucinich's most likely prospect for an academic theorist who actually benefited from the radical fountainhead. In the end, however, we find Vucinich chiding Kistiakovskii for not paying sufficient attention to Mikhailovskii's works (pp. 138-39).

I confess that I am far from comprehending anything as broad as Russian social thought in any period, but it seems to me that Vucinich has ignored a number of original and important social theorists. On the one hand, there were the legal scholars. Only a few of these appear in Vucinich's account; Gradovskii, Korkunov, and Lazarevskii are never mentioned. It is not true, as Vucinich implies, that these men were "official ideologues" wallowing in metaphysics. On the contrary, their considerations regarding what may be called the sociology of administration, like those of Max Weber and Lorenz von Stein, probably benefited from their suspicion or outright rejection of the transparent quackery about individualistic solidarity (or collective individualism) that occupied the imaginations of contemporary ideologues. On the other hand, Vucinich ignores a number of Neo-Populist theorists who produced some of the most significant studies of peasant society and rural social development in the world. I am thinking especially of V. S. Prugavin, A. A. Chaianov, and P. P. Semenov, but there were others. I doubt that these men burned much lamp oil studying the turgid metaphysics of Chernyshevskii and Kropotkin, among others, but they were social theorists just the same. Perhaps Vucinich felt a need to account for the decline of Populist ideology, and his thesis forbade him to imagine that Populist scholarship, like any scholarship, could prosper on its own apart from the fortunes of politics—until 1930, of course, when individualistic solidarity at last imposed itself on academe.

It is not my purpose to criticize Vucinich merely for missing this or that theorist; I would not have him catalog every commentator on Russian social thought. The discussions he offers are of interest, and in any case it is a tribute to his scholarship that his evidence often ranges well beyond the structure he has imposed upon it.

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G. S. ZHUKOV. *Peterburgskie marksisty i gruppa "Osvozhdenie truda"* [Petersburg Marxists and the Group "Emancipation of Labor"]. Leningrad: Lenizdat. 1975. Pp. 322. 56 k.

Until the recent publication of G. S. Zhuikov's book, post-Stalinist Soviet historiography of Rus-

sian Marxism in the 1880s has been limited to Iu. Z. Polevoi's classic *The Origins of Russian Marxism* (Moscow, 1959), R. A. Kazakevich's *Social-Democratic Organizations in Petersburg* (Leningrad, 1960) and S. Ovsianikova's *The Blagoev Group* (Moscow, 1959). While Polevoi's study is certainly the most detailed, all three works suffer from heavy reliance on Lenin's pronouncements on the subject, insufficient attention to archival sources, and highly selective use of memoir literature. Drawing extensively from the archives of the Ministry of Justice and the Department of the Police, Zhuikov avoids many of the distortions of his colleagues and presents valuable new material which documents the emergence of Russian Marxism in the 1880s.

Zhuikov divides his book into five chapters, the first of which traces Plekhanov's career as a *narodnik* in the 1870s. The second chapter discusses Plekhanov's break with *narodnichestvo* as well as Marx and Engels's views of the Russian revolutionary movement. These chapters and the final one, intended "to demonstrate the direct influence of V. I. Lenin on the activity of the group Emancipation of Labor" (pp. 11-12), do not depart from the conventional Soviet interpretations. It is in Chapters 3 and 4 on the influence of the emigré Emancipation of Labor Group on the development of the revolutionary movement inside Russia that Zhuikov presents interesting new archival evidence to support his overall thesis that Plekhanov and his group directly contributed to the ideological maturation of the Blagoev, Tochisskii, and Brusnev-Kashinskii groups at home. In addition, the author successfully ties these proto-Marxist groups to the dozens of small but influential student societies and underground publications which, during the 1880s, demonstrated an increasing devotion to the foundation of a Russian Social-Democratic party as well as to the popularization of Marx's works.

The weaknesses of Zhuikov's argument derive primarily from his dogmatic commitment to proving that Plekhanov alone brought Marxism to Russia. As a result, he dismisses the 1920s work of N. L. Sergievskii who argued not only that the domestic Russian groups of the 1880s came to Marxism on their own, but also that *they* influenced Plekhanov. The author also discounts other important aspects of the development of Russian Marxism in these years: the Marxist trappings of several sub-groups of "People's Will," the impact of the Polish Marxists of the "Proletariat," and the increasing discussion of Marx's works in independent workers' groups. Still, Zhuikov's work is an important and lasting contribution to the study of the revolutionary movement of the 1880s and the obscure origins of Russian Marxism.

NORMAN NAIMARK  
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MARTIN A. MILLER. *Kropotkin*. Chicago: University of Chicago Press. 1976. Pp. x, 342. \$15.00.

Martin A. Miller, who recently edited a useful collection of Kropotkin's writings, has now produced the first full-length biography of Kropotkin in English since George Woodcock and Ivan Avakumović published *The Anarchist Prince* in 1950. In many ways it is a solid achievement. Based on a wide range of printed and manuscript sources, including a rich vein of Soviet archival materials previously untapped by Western scholars, it treats with sympathetic insight the career of a major revolutionary thinker whose libertarian doctrines have been enjoying an impressive revival and whose numerous books have been reissued in several languages. Among its other virtues, the study provides an interesting discussion of Kropotkin's formative years prior to 1876, when his dramatic escape from confinement made his name an object of admiration within radical groups throughout Europe. Because of this, it constitutes a welcome addition to the literature on Russian Populism as well as on the anarchist movement with which Kropotkin was afterward associated.

Why then does one close this book with a feeling of disappointment? For one thing, the story for the most part is well-known. For all his research, Miller has turned up little that is unfamiliar to the specialist or that will alter the accepted picture of Kropotkin, either as a private individual or as a revolutionary leader. The book, moreover, is marred by errors of fact and interpretation. For example, Kropotkin wrote not two but some twenty letters to Lenin (not to mention others to lesser Soviet officials) criticizing the Bolshevik dictatorship. No wonder, according to some reports, Lenin asked impatiently when the "old fool" would stop pestering him. Furthermore, the author's style is undistinguished and at times ungrammatical (the chapter on Kropotkin's social theory is particularly heavy-handed), lacking not only the grace of Kropotkin's own celebrated memoirs but also the clarity of Woodcock and Avakumović's pioneer work, which, despite its inadequate documentation, remains unsurpassed.

This, then, is far from being a definitive biography, as its publishers describe it. Although its merits are considerable, Miller's book fails to convey the powerful impact of Kropotkin's ideas during his lifetime or their enduring vitality after his death. Kropotkin's greatness does not come through, and so the reader is left to wonder why he inspired, and continues to inspire, such intense and widespread devotion.

PAUL AVRICH  
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City University of New York

G. L. SOBOLEV. *Revoliutsionnoe soznanie rabochikh i soldat Petrograda v 1917: Period dvoevlastiia* [The Revolutionary Consciousness of Petrograd Workers and Soldiers in 1917: The Period of Dual Power]. (Akademii Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1973. Pp. 327.

Bearing in mind the key importance of Petrograd factory workers and military personnel for the success of the Bolsheviks in the October Revolution, it is ironic that Soviet historians have not more systematically investigated the political consciousness and social psychology of the Petrograd masses in 1917. Drawing upon the 1917 press, a rich body of published and unpublished memoirs and documents (among the latter such previously largely untapped sources as church archives and personal letters to the Petrograd Soviet), G. L. Sobolev has to some extent helped to fill this hiatus. His generally informative, interestingly written study begins with a detailed examination of the political and social outlook of Petrograd workers and soldiers during and in the immediate aftermath of the collapse of the tsarist regime. (Despite their considerable influence on events in Petrograd, Sobolev regrettably omits the thirty thousand sailors of the nearby Kronstadt naval base from his analysis.) Subsequent chapters are devoted to the campaigns of the various Russian political groups for the allegiance of workers and soldiers in the winter of 1917, and to the reasons for and scope of popular support for transfer of power to the soviets in the spring and early summer. The book focuses on the formative, so-called "peaceful" period of the Revolution; it ends with the suppression of the abortive July 1917 uprising.

When describing the behavior of the Petrograd masses in the course of the February Revolution, Sobolev tends to magnify the leadership of the Bolsheviks. Unlike many earlier Soviet works, however, this book does not exaggerate the degree of influence and authority enjoyed by the party during the first weeks of March (most factories and military units, for example, picked moderate socialists as deputies to the first Petrograd Soviet). The author attributes this initial Bolshevik weakness to such factors as the wartime influx of peasants into the capital (both to replace veteran workers sent to the front and to serve in the military), as well as the failure of most workers and soldiers, in the euphoric first weeks after the February Revolution, to perceive their class interests and the degree to which the Bolsheviks alone represented them. One need not fully agree with these conclusions to profit from Sobolev's consideration of this issue.

This book persuasively demonstrates that, early Bolshevik difficulties notwithstanding, by the end

of June 1917 support among the Petrograd masses for the chief Bolshevik political slogan "All Power to the Soviets!" was overwhelming. Sobolev's account of the evolution of popular concerns between February and July 1917 suggests that the alienation from the Provisional Government of most workers and soldiers, and their orientation toward the creation of a Soviet regime, was a gradual process, probably due as much to dissatisfaction with the military and domestic policies of the coalition cabinet and disappointment with the results of the February Revolution, as to the organizational and agitational efforts of the Bolsheviks.

The author is less helpful in dealing with issues in which the facts do not fully conform to officially prescribed ideological assumptions. His book is better than any previous study in identifying many of the influences that shaped mass public opinion and in clarifying important, often overlooked distinctions in the personal concerns and political views of workers and peasant-soldiers. But when directly comparing the behavior of workers with that of soldiers, Sobolev goes well beyond the available evidence to idealize the former. In his account, the political maturity, discipline, and significance of workers, relative to soldiers, are vastly exaggerated.

Similar weaknesses are reflected in the author's treatment of problems bearing on such sensitive a priori assumptions as the infallibility and supremacy in political matters of Lenin, the disciplined, essentially unified character of the Bolshevik party in 1917, and the top party leadership's paramount position in providing inspiration and direction for the October Revolution. For example, Sobolev aptly observes that the psychology of the revolutionary masses exerted an influence on party agitators. But perhaps because of his concern with documenting the exclusive role of Lenin and the higher party leadership in masterminding revolutionary strategy and tactics, the author does relatively little to illuminate the ways in which popular opinion may have helped shape Bolshevik policy.

Sobolev also discusses the strong inclination toward moderation on the part of many important Bolsheviks before Lenin's return to Russia in early April; conversely, in describing events immediately preceding the July uprising, he alludes to the great impatience of part of the leadership of the Bolshevik Military Organization for immediate revolutionary action against the government. However, he implies that both tendencies were effectively stifled by Lenin and had little subsequent impact. In a lengthy account of the background of the July uprising, the author neglects evidence that leaders of the Bolshevik Military Organization and the Petersburg Committee, responsive to their militant constituencies, encour-

aged the insurrection against the wishes of Lenin and a majority of the Central Committee. The Bolshevik Military Organization spokesman A. Ia. Semashko, one of the chief instigators of the rebellion, is not mentioned at all. Sobolev suggests that Military Organization activists made every possible effort to hold back the rebellion, failing to take note of such evidence to the contrary as the admission of the Military Organization official V. I. Nevsky that Military Organization personnel initiated preparations for a possible insurrection weeks before the July days and that on July 3, upon being dispatched to persuade the soldiers to remain in their barracks, he, for one, talked to them "in such a way that only a fool could come to the conclusion that he should not demonstrate."

ALEXANDER RABINOWITCH  
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VIRGIL D. MEDLIN and STEVEN L. PARSONS, editors.  
*V. D. Nabokov and the Russian Provisional Government, 1917.* With an introduction by ROBERT P. BROWDER. New Haven: Yale University Press. 1976. Pp. viii, 188. \$12.50.

Six months after the October Revolution, V. D. Nabokov, a prominent Russian liberal and the father of the famous writer, jotted down his reminiscences of the Provisional Government in which he had served as head of chancellery. Working without notes or documents he concentrated on characterizing the various government figures he had known, and he produced a fine, short essay of lasting value. It appeared in Russian in 1922 and has been used subsequently by all serious writers on the Russian Revolution, Western and Soviet alike. Virgil Medlin and Steven Parsons have now made it available to English readers in an excellent annotated translation, which includes the essay "V.D. Nabokov in 1917," by Baron B. E. Nol'de, and a lucid, intelligent introduction by Robert Browder.

Nabokov epitomized much of Russian liberalism in this period. An intelligent, compassionate jurist (whose father had served for a time as tsarist minister of justice), he abhorred violence, and watched with growing consternation as the legal foundations of Russian society began to crumble. Though not an activist (from February 28 until March 2 he stayed in his apartment) or even a major participant in the Provisional Government, he felt "a sense of spiritual elation" at the thought that "the people had cast off their chains, that despotism had collapsed"; he was ready to accept his and his party's share of responsibility for subsequent events. He also challenged the "classical imperialism" of Kadet leaders, such as his close friend

Miliukov, "the most important figure, intellectually and politically, in the first Provisional Government," and Nabokov felt his views on the war made conflict "utterly inevitable." But Nabokov's tendency to see the Revolution essentially as a political process, rather than a vast social upheaval as well, and his insistence that common Russian soldiers "had the usual senseless, vacant, and malevolent faces," also reflect the fatal elitism of liberal Russia, and an essential aspect of his party's political ineptitude.

Nabokov is at his best in his scathing appraisal of democratic Russia's lackluster leadership, which takes up a full two-thirds of his essay. He regarded Kerensky as "a stupid joke," prone to posturing and panic; and Prince Lvov as "weak" and "indecisive." His left Kadet colleague Nekrasov was "ambitious," but "one of the few outstanding men of 1917" (a view radically different from that held by many of his fellow Kadets); Steklov was "impudent" and a "boor"; and Tsereiteli was "absolutely helpless" with anything that required "deep insight, individuality, or independent thought." If readers learn little about the actual course of events from Nabokov (he concentrates almost entirely on the first two months of the Revolution and then, briefly, on the October coup), he provides much to think about in terms of Russia's revolutionary personalities. His brutal death in 1922 at the hands of proto-Nazi Russian assassins stilled an insightful voice.

The editors have wisely prepared this volume for the general reader with useful bio-bibliographical footnotes and a comprehensive index.

WILLIAM G. ROSENBERG  
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Ann Arbor

ROBERT H. MCNEAL, general editor. *Resolutions and Decisions of the Communist Party of the Soviet Union*. Volume 1, *The Russian Social Democratic Labour Party, 1898–October 1917*, edited by RALPH CARTER ELWOOD; volume 2, *The Early Soviet Period: 1917–1929*, edited by RICHARD GREGOR; volume 3, *The Stalin Years: 1929–1953*, edited by ROBERT H. MCNEAL; volume 4, *The Khrushchev Years, 1953–1964*, edited by GREY HODNETT. Buffalo: University of Toronto Press. 1974. Pp. xxxi, 306; xi, 382; x, 280; x, 328. \$75.00 the set.

The editors of these volumes have performed a notable service by providing students and scholars with a readily accessible collection in English of the scattered decisions and resolutions of the Russian Communist Party from its inception to the end of the Khrushchev era. Selected from a wide-ranging list of sources, the materials are informatively introduced and meticulously documented and annotated.

It could not have been an easy assignment, nor, for the most part, a stimulating one, as the dedication in the first volume attests: "To the late Vladimir Ilyich Lenin, who summed up the feelings of the present editors, upon this task, when he wrote to Maxim Gorky in 1913: 'Resolutions, they say, are the most boring of all forms of literature. I am a man who has consumed too many resolutions'." Indeed, it would be difficult to achieve a more pedestrian style or employ more obfuscating language than the Social Democrats and especially the Bolsheviks practiced over the years in formalizing their disputes and decisions. Yet, these documents contain vital information and important keys to the workings of the party and, later, of the state.

As one who has participated in a somewhat similar enterprise, this reviewer is well aware of the criticism that always attends such an ambitious publication. Why was this document included and that not? What was omitted when ellipses appear (which is seldom)? How do we know that other documentation may not exist which is unknown to the editors and may be far more significant than those included? The answer to these and other questions that inevitably arise upon perusal of these volumes is found in our knowledge of the editors' scholarship, its reflection in the contents, and the clear delineation in the introduction of their methods and delimitations. It is reassuring. We are told foreign policy materials are not included and that the term "decisions" is narrowly defined. Until the irrevocable break of 1917, Menshevik, as well as Bolshevik materials are printed; but after that date "the" party only is represented. Certainly, we are already cognizant of the need to supplement such materials with government documents.

The editors should be congratulated for their industry and high standards.

ROBERT PAUL BROWDER  
University of Arizona

ANTON I. DENIKIN. *The Career of a Tsarist Officer: Memoirs, 1872–1916*. An annotated translation from the Russian by MARGARET PATOSKI. Minneapolis: University of Minnesota Press. 1975. Pp. xxii, 333. \$17.95.

Anton Ivanovich Denikin was one of the most extraordinary generals in modern Russian history, not because of his battlefield exploits (although there were some) but because of his role during a major political turning point, the Russian Civil War (1918–21). Denikin has written a remarkable five-volume history of that war, *Ocherki russkoi smuty*, which is comparable in both literary excellence and documentation to Churchill's history



of World War II. Yet few, even among scholars, are familiar with Denikin's writings (which include several shorter memoirs and commentaries as well), knowing him only as the unsuccessful leader of the White forces fighting the Reds after the Bolshevik seizure of power. It is fortunate that some of Denikin's work has been made available in English, not only because of his importance in the history of the period but also because of his importance as a historian.

To the brief summary translations of the *Ocherki* published in 1922 and 1930 has now been added a full translation of the memoir of his childhood and military career to 1916. This, the last of his books, is the most personal of all his writings, but it is not without significance as a contribution to the history of the period. His childhood in a mixed Russian-Polish (and Orthodox-Catholic) family of humble origins; his rise against no small odds through the ranks of the military, where he learned honor, courage, and discipline but also saw much corruption, incompetence, and arbitrariness; and especially the experiences, both military and political, in 1904-05 and in the frustrating days of the first two years of the world war—all are related in anecdotal accounts that convey in the most succinct form insights into Russian history in one of its most fateful periods.

Margaret Patoski has done a careful and commendable job of translating, and she has added an introduction reviewing Denikin's life and an appendix of biographical sketches of the most important persons mentioned in the book. She has also notably assisted the nonspecialist reader by greatly supplementing Denikin's original footnotes and including a brief bibliography. This is, then, a well-done and very welcome addition to Russian memoir literature now available in English. I have only one petty complaint: the change in the title from Denikin's "Russian officer" to "tsarist officer." It was probably thought that "tsarist" would more clearly convey the pre-Revolutionary setting, but in view of the implication of reactionary views that this adjective has carried since 1917 and Denikin's long struggle as a moderate liberal to overcome that label, it seems an ironic blow from his translator.

GEORGE BRINKLEY  
*University of Notre Dame*

DIMITRY V. LEHOVICH. *White Against Red: The Life of General Anton Denikin*. New York: W. W. Norton and Company. 1974. Pp. 556. \$12.50.

To most people who knew General Anton Denikin, he was an honest, competent officer with moderate political views. The military reforms of the late nineteenth century had allowed such men of

humble origins to rise in the ranks on the basis of merit. After the October 1917 Revolution, Denikin was thrust into a role demanding a forceful personality eager and able to lead less in a military than in a political sense. Unfortunately for the anti-communist cause, neither man who led the two most important White movements, Denikin and Admiral Kolchak, measured up to the task. Historians generally have been sympathetic to their human qualities, but critical of their political ability in such highly political roles.

The author of this biography, Dmitry Lehovich, is neither a journalist nor historian. He served as a young recruit under Denikin, but they did not meet until later in life. Lehovich seems to have written this book after a lifetime of asking, "Who was this man whom I followed? For what were we fighting? Why did our cause fail?" He never uses the first person, but his experience enlivens the story and, as might be expected, the author gives Denikin a very sympathetic, though not uncritical, hearing. Lehovich is conscious, for example, of Denikin's failure to deal effectively with separatist movements and the land question, but he fails to analyze adequately these questions and Denikin's failure as a political leader. Nevertheless, this work can be read with profit by specialist and general reader alike. It is one of the very few biographies of a Civil War leader and will not soon be replaced. Lehovich has well researched Denikin's life and has judiciously placed it into the larger perspective of war and civil war. Furthermore, he continues the story beyond where a trained historian might stop by describing the life of Denikin and his wife in exile. His account of emigré politics is fascinating, and his description of the Denikins in Nazi-occupied France is moving. It is a pity that this book can not be read widely in Denikin's homeland where the record has been so distorted.

CANFIELD F. SMITH  
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Birmingham*

CANFIELD F. SMITH. *Vladivostok Under Red and White Rule: Revolution and Counterrevolution in the Russian Far East, 1920-1922*. (Publications on Russia and Eastern Europe of the Institute for Comparative and Foreign Area Studies, number 6.) Seattle: University of Washington Press. 1976. Pp. xv, 304. \$11.00.

Canfield F. Smith has examined a chapter of history which has received inadequate attention from Western scholars. Smith's monograph—an outgrowth of his dissertation—rests on an impressive array of archival and official sources, eye-witness accounts and recollections, and secondary treatments. The book covers the period 1920-1922 and



focuses on the major political and institutional developments and leading personalities in Vladivostok and the Maritime Province during the turbulent period from Admiral Kolchak's defeat to the collapse of Vladivostok's last White government in late 1922.

Smith analyzes the major contending political groups, particularly the region's communists, and the coalition governments they formed against the background of Japan's intervention and scheming in the Far East, the dislocations produced by the Civil War, the elusive search for solutions to the Far East's political dilemma by successive governments' frustrating relations with occupation authorities, Czech Volunteer Corps leaders, the Far Eastern Republic buffer state, and major local forces (Semenovites, Kappelites, partisans, Bolsheviks, etc.). Vladivostok, virtually the last anti-Bolshevik refuge, is portrayed as a hapless and feeble bystander in the power struggle among Japan, Soviet Russia, and the United States.

The work contains a number of factual errors, technical flaws, and stylistic idiosyncracies (NEP adopted in 1920; St. Petersburg for Petrograd; late identification of individuals and institutions). The author should have introduced more background material, and the first two chapters should have been arranged more coherently before commencing the main narrative. A more expansive treatment of the changing socio-economic conditions impinging on political events would have been useful as would further detail about indigenous political forces. The author applies largely traditional explanations to the Bolsheviks' victory and the Whites' defeat in Vladivostok and its environs; he suggests unconvincingly that the Whites might have been more successful (especially if they had had a Napoleon).

Nevertheless, this book is a welcome and useful addition to the specialized literature of the period, offering new information and insights, and extending John F. N. Bradley's recent brief survey of the Russian Civil War.

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University of Wisconsin,  
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V. A. DUNAEVSKII. *Sovetskaia istoriografiia novoi istorii stran Zapada, 1917-1941 gg.* [Soviet Historiography of the Modern History of Western Countries, 1917-1941]. (Akademiia Nauk SSSR, Institut Istarii SSSR.) Moscow: Izdatel'stvo "Nauka." 1974. Pp. 375. 1 r. 87 k.

This well-conceived book surveys and examines the chief writings on modern European and American history by Soviet historians, Marxists and

non-Marxists, before World War II. It is based on extensive reading of monographic literature and on what seems to be careful study of a large amount of archival materials, including funds of the Central Party Archives. Its principal virtue reflects the author's ability to classify and present a large mass of information. Its principal defect is the author's tendency to concentrate on the conclusions of investigations and to give scant attention to problems of source collection and analysis, research technique, and popularization.

Dunaevskii presents Lenin's observations about modern European and American history in the first chapter. In successive chapters he analyzes studies of "bourgeois revolutions" (the Glorious Revolution, the French Revolution, European revolutions of the 1830s and 1840s, the American Civil War, and the Paris Commune), socioeconomic history and the evolution of the working class and socialist movement to World War I, West-European "utopian" (pre-Marxist) socialism, and international relations. Chapter six examines historiographical studies of the subjects listed above. Finally, chapter seven provides a description of the research and pedagogical institutions in which most of the studies analyzed by the author were undertaken.

If viewed as a manifestation of the effort by Soviet scholars to set the history of the Soviet Union on a scholarly foundation, this book is another sign of retrogression. The author, in a work published in 1966, did more than any other scholar to clarify the context of Stalin's intervention in the historical profession. (See "Bol'sheviki i germanskii levye na mezhdunarodnoi arene [Nekotorye aspekty temy v osveshchenii sovetskoi istoriografii kontsa 20-kh-nachala 30-kh-godov]" in *Evropa v novoe i noveishee vremia* [Moscow, 1966], pp. 491-513.) The present work contains only a vague and seemingly favorable reference to this matter. In the realm where Dunaevskii's own personal judgments seem to predominate, and not political constraints, he reveals an injudicious turn of mind. My caveat is against the author's practice of sorting out historical figures into good and bad. Because he either idealizes leading historians, or almost wholly denigrates them, he produces caricatures. The guiding idea of this work is the rise and triumph of Marxist historical scholarship, a theme the author shares with all Soviet scholars now writing on historiography. The book lacks, however, a thesis or a distinctive point of focus. The author does not express any idea of his own as to how Marxist historical scholarship unfolded with respect to the specific subject matter he has examined.

GEORGE ENTEEN  
Pennsylvania State University

VERA T. RECK. *Boris Pil'niak: A Soviet Writer in Conflict with the State*. Montreal: McGill-Queen's University Press. 1975. Pp. 243. Cloth \$12.50, paper \$6.00.

In the prologue to this study, which focuses on Pil'niak's scrape with authorities in 1926 and on the campaign against him in 1929, Vera Reck notes that the two events were "purely political" and were literary only in the sense that they "had at their core" two of Pil'niak's works of fiction. The author's inability to decide whether the literary or the political is the more important is the basic weakness of the method used in her study. Although it becomes quite evident, in terms of the author's own analysis, that the content of Pil'niak's work was, at best, only a secondary cause of the attacks on him, Reck devotes the first half of her work almost entirely to a political-literary analysis of "The Tale of the Unextinguished Moon" and *Mahogany*. Only in the second half of the volume does the author turn her attention to the "purely political" factors that were the underlying causes of the campaign of 1929. Here she finally makes it clear that the main purposes of the attack on Pil'niak were to combat *apolitizm* among writers and to bring about a reorganization of the All-Russian Writers Union.

The author's method—which is to move from the most speculative and inconsequential to the essential, with many digressions into the irrelevant along the way—tends to obscure the main sequence of developments and their significance. Paradoxically, the scant four pages that Alex Shane gives to the anti-Pil'niak campaign in *The Life and Works of Evgenji Zamiatin* indicate more clearly and concisely the "purely political" significance of the Pil'niak affair of 1929 than do the many pages that Vera Reck devotes to it. (Shane's work is nowhere mentioned in the volume under review.)

One must be grateful to Vera Reck, whatever the deficiencies of her approach, for ferreting out hitherto unknown facts about Pil'niak's career. Particularly valuable are her reports of interviews with Pil'niak's contemporaries.

E. HAROLD SWAYZE  
University of Washington

M. V. VARFOLOMEEVA. *Rol' massovykh bibliotek v kul'turnoi revoliutsii v SSSR (1928-1941 gg.)* [The Role of the Public Library in the Cultural Revolution in the USSR (1928-1941)]. (Akademiia Nauk SSSR, Biblioteka AN SSSR.) Moscow: Izdatel'stvo "Nauka." 1974. Pp. 230.

As the history of the Communist movement shows, libraries are institutions of great importance. They were the sources from which Marx and Lenin

drew information about the contradictions of capitalism on which they built their systems. In the Soviet Union the library is an agency for the instruction of the masses in the views of communism.

In spite of the lack of a bibliography and index and despite equivocations as to some of the political and cultural events between 1928 and 1941, this work is an informative account of the process by which "library affairs in the years of the pre-war Five Year Plans found themselves under the unremitting attention of the Central Committee of the All-Union Communist Party (Bol'shevik) and of the Soviet government." One may also draw from the book a clear realization of the official Soviet view of libraries—and, by extension, of other media of information—as instruments for providing the public with concise and unambiguous answers to the problems of the day, free from any taint of St. Augustine's "Audi partem alteram." Furthermore, although the absence of the usual scholarly apparatus reduces the bibliographic value of this volume, the footnotes refer to some of the professional library literature of the period that can be of interest to those who wish a closer look at the implementation of Soviet policy. Despite the practical usefulness of Varfolomeeva's work for the student, it is, however, disheartening to realize that the Soviets see belles-lettres (there is scarcely any other fitting translation of *khudozhestvennaia literatura*) chiefly as a "weapon of party propaganda and agitation." Are Tolstoy's Natasha Rostova and Gogol's Khlestakov really only elements in the analysis of the "decay of the feudal-serf system"?

R. V. ALLEN  
Library of Congress

IU. V. BORISOV *et al.*, editors. *Dokumenty vneshnei politiki SSSR* [Documents of the Foreign Policy of the USSR] Volume 18, *1 ianvaria-31 dekabria 1935 g.* [January 1-December 31, 1935]. (Ministerstvo Inostrannykh Del SSSR, Komissiiia po Izdaniuu Diplomaticheskikh Dokumentov pri MID SSSR). Moscow: Izdatel'stvo Politicheskoi Literatury. 1973. Pp. 717.

By now the format and editorial approach in the *Dokumenty vneshnei politiki* (DVP) are well established. Each volume covers a single year, providing a number of previously unpublished documents from the diplomatic archives as well as the full texts of some of the more important diplomatic agreements. No consideration is given to the activities of the Comintern, attention being narrowly focused on the actions and policies of the Commissariat of Foreign Affairs. High-level party and state foreign policy documents are customarily limited to extracts from previously published reports by government or party leaders to official gatherings.

The eighteenth volume of *DVP* follows the established pattern but introduces some previously unpublished reports from archival sources of high-level diplomatic conversations. Of these the most important is a report of a meeting on March 29, 1935, at which J. V. Stalin and V. M. Molotov met with Britain's Anthony Eden, who at that time held the post of Lord Privy Seal, together with Ivan Maisky, Soviet Ambassador to London, his British counterpart, Viscount Chilston, and the head of the League of Nations section in the Foreign Office, William Strang.

As reported in *DVP 18*, the conversation, on J. V. Stalin's initiative, centered on the danger of Nazi aggression and the possibilities of countering it through a multilateral mutual security pact. In words that sound like a prophetic condemnation of his own policy toward Hitler's Germany four years later, Stalin is quoted as denying the value of a bilateral nonaggression pact with Germany on the ground that there would be no guarantee that Germany would honor such an agreement. Later in the conversation Stalin casts another long shadow into the future—this time to the purge of the Red Army leaders in 1937—by stating that the Germans have been circulating rumors that Red Army Marshal Tukhachevski has been conspiring with Hermann Göring in plans for a joint Soviet-Nazi attack on France.

One may well raise the question why the editors of *DVP 18* included this highly significant document. It may be surmised that they did it in part to provide evidence of Stalin's concern with German aggressive tendencies as early as 1935, and thereby to help exculpate him from charges of blindness to that danger in 1939. It is worth noting that the text of the official British report on the conversation has been available for some time, having been published in the official series, *Documents on British Foreign Policy, 1919-1939* (second series, vol. 12, pp. 766-69). Thus the usual Soviet demand for the preservation of secrecy, which ordinarily guides the editors of *DVP*, could for once be relaxed. At the same time the possibility is raised of a useful comparison between the Soviet text and that in the British diplomatic archives. The two are by no means identical in overall structure or in detail.

The same explanation cannot, it would seem, be invoked to explain a second precedent-breaking inclusion in *DVP 18*, that of a report of a conversation between Tukhachevski and General Henri Loiseau, deputy chief of staff of the French Army, which took place on September 25, 1935. Throughout this conversation Tukhachevski manifests a lively concern with French military weakness and the resulting vulnerability of France to German aggression. Displaying a strikingly prophetic grasp of the shape of German strategy in the campaigns

of 1940, Tukhachevski sounds out Loiseau on the state of military liaison between France and England, calling the French official's attention to the dangerous situation that would result in the event of a German attack on France following German occupation of Belgium and the Netherlands.

Adding still further complexity to the picture of Soviet-German relations in 1935, *DVP 18* publishes a number of reports from Berlin by David Kandelaki, a Soviet official whose task was to negotiate a trade pact with Nazi economic official Hjalmar Schacht, but who is widely believed to have been sounding out the Nazi leaders, on behalf of Stalin, as to the possibility of a political deal.

By their inclusion of these important documents the editors of *DVP 18* have shown, perhaps inadvertently, how much more valuable the series could be to the historian. One can only hope that in future volumes the editors will be able to continue and expand the precedent established here. Not to do so, at a time when other nations are making their diplomatic archives before 1939 available to unfettered historical research, would invite unflattering comparisons about the status of historical studies in the Soviet Union. Even as it now stands, however, the series deserves close scrutiny by all historians concerned with international relations during this period.

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A. A. GRECHKO *et al.* *Istoriia vtoroi mirovoi voyny, 1939-1945* [History of the Second World War, 1939-1945]. In twelve volumes. Moscow: Izdatel'stvo "Voennoe Izdatel'stvo Ministerstva Oborony SSSR." Volumes I, II, III. 1973; 1974; 1974. Pp. 367; 479; 503. 2 r. 80 k. per volume.

The first three volumes (there are twelve planned) of *Istoriia vtoroi mirovoi voyny, 1939-1945* deal with the period from the end of the First World War to the eve of the German attack on the Soviet Union in June 1941. Publication of the entire work in an edition of 330,000 copies is under the supervision of a prestigious editorial commission chaired by the late defense minister A. A. Grechko. According to an introductory note from the commission, one of the principal tasks of this major effort is to expose "bourgeois falsifications" of the history of the Second World War. The one East German and the fifty Soviet authors have endeavored in these first volumes to correct Western accounts of the prewar period that, they claim, ignore capitalism's responsibility for the rebirth of German militarism, that deny that fascism is the child of imperialism, and that even try to place the blame for the outbreak of war on the Soviet Union.

This attempt at historical justification will not

convince Western readers, not only because Westerners cannot accept the grotesque straw men created to represent the collective views of Western historians, but also because these volumes make it so glaringly apparent that the Soviets (not just historians) cannot yet come to grips with the facts of their own history. Specifically, the attempt to cover up the deeds and misdeeds of J. V. Stalin in accordance with the current party line severely damages the credibility of the work. Stalin, the key decision-maker in the Soviet state during most of the period, is mentioned only twenty-four times in over 1,200 pages of text. His prewar rank or position is never mentioned. In seven cases his name is but one in a list of individuals who made an important contribution to some achievement of the Soviet Union. Ironically, in one of these cases he is included in a listing of those who made a valuable contribution to Soviet military science in the 1930s, along with A. I. Yegorov and M. N. Tukhachevski, whom he had shot in 1937 (p. 171, vol. 2). The purges of 1937-38 are treated in one paragraph that deserves full translation: "In 1937-38 as a consequence of baseless accusations, a significant number of commanders and political workers were released from the army. On the basis of a decision of the January (1938) Plenum of the Central Committee of the party, a special commission was established in the NKO [People's Commissariat of Defense] for investigating the complaints of those released from the army. It established that many of them were removed from their positions without the required grounds. It was possible to correct the errors in the work with military cadres to a large degree and achieve, before the Second World War, the return of military cadres of commanders and political workers devoted to the Motherland and the Communist party" (vol. 2, p. 206).

Considering the torrent of bitter recollections of the role of Stalin and his henchmen in the 1937-38 purges that were published in the Soviet Union during the Khrushchev de-Stalinization campaign, one wonders how well that paragraph will be received by a Soviet reader familiar with these sources. Obviously, the present regime is confident that the great majority of the Soviet readership will accept it without protest, since Soviet histories of the war have been bowdlerized recently to remove all but the most innocuous criticisms of Stalin. This treatment of Stalin appears to be necessary if the "all-wise" Communist party of the Soviet Union that glorified him is to continue to claim omniscience in the past, present, and future. This version of the purges also avoids the question of the competence of the Soviet military high command and the effectiveness of the Red Army after its most able leaders had been jailed or executed.

Without this background it becomes more difficult to understand the doubts Western democracies had of Stalin as a political leader and of the military capabilities of the Soviet state. Their reluctance to conclude an alliance with the Soviet Union against the Axis then appears, as the Soviets now claim, to have been a grand design to turn Hitler to the east.

Compared to the brazen treatment of the purges it seems picayunish to also report that the volumes contain no mention of the Soviet-German military cooperation between 1923 and 1935, which enabled the Wehrmacht to evade the restrictions of the Treaty of Versailles and train aviation and armored specialists in the Soviet Union. However, this omission, combined with the treatment of Stalin and his purges, provides an additional basis for challenging the Soviet authors on the very points on which they have sought to "expose" Western history of the period. Thus, does not the evidence show that the Soviets themselves contributed to the rebirth of German militarism? And if, as the Soviets insist, fascism is the product of imperialism, was not Stalinist totalitarianism the child of socialism in one country? And, does not Stalin and the system that produced him bear a full share of responsibility for the events which led to the outbreak of the Second World War?

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RALPH CARTER ELWOOD. *Russian Social Democracy in the Underground: A Study of the RSDRP in the Ukraine, 1907-1914*. (Publications on Social History, number 8. Issued by the Internationaal Instituut voor Sociale Geschiedenis.) Assen: Van Gorcum & Co. BV. 1974. Pp. xix, 304. 67.60 gls.

This study re-examines the role and activities of the illegal Social Democratic Labor party during the years of its decline following the first Russian Revolution of 1905-06. Ralph Carter Elwood sought to achieve his goal through analysis of the composition, organization, and activities of the party, using as sources a great deal of printed material, such as illegal pamphlets, leaflets, programs, and contemporary periodicals and newspapers. Chapters dealing with 1907-11 follow an introductory review of the Ukraine and its revolutionary movement. The final chapter and epilogue concern changes occurring in the last prewar years and during the war and Revolution in Russia and the Ukraine. The author also notes the parallel developments of the party's activities abroad.

A reworked Ph.D. dissertation, the book shows some traces of its origin. It contains a lot of useful detail, though the encyclopedic nature of the information sometimes affects the readability of the



volume. The same may be said of the rather lengthy introductory information (ways of printing pamphlets and leaflets, distribution of papers, and so forth). Moreover, Elwood seems to indulge in weak generalizations and hypotheses. For instance, the idea that Jewish merchants unintentionally caught the peasant in the classic "scissors" by buying grain cheaply for export, and selling him expensive manufactured goods, has little or no basis in fact. (The average peasant had virtually no grain to sell; he bought little besides matches. Low prices resulted from foreign competition and the poor quality of grain.) Again, the implied hypothesis that with different national and land policies the RSDRP could have had a better-organized party and "their own October revolution" in the Ukraine is a baseless speculation. Largely illiterate (more than eighty percent), the peasants could hardly form the backbone of a well-organized party bound by an ideology. They were more likely, rather, to form anarchic bands fighting on one side today and on the other tomorrow. Whole Ukrainian "red regiments" readily went over to the Central Directory or split to follow this or that Attaman or pogrom leader, sometimes returning to hoist the Red flag. Nor did their over two million votes for the SRs have any significance. Finally, bolshevism had to be imposed in the north of Russia as well as in the south. Elwood himself points out that "Lenin and the SDs had little success among the Russian peasantry." They had to impose bolshevism on them, too, by terror and the Red Army.

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ALEXANDER FISCHER. *Sowjetische Deutschlandpolitik im Zweiten Weltkrieg, 1941-1945*. Stuttgart: Deutsche Verlags-Anstalt. 1975. Pp. 252. DM 28.

More than any other country, Germany has been a graveyard of Soviet hopes. When after 1917 the Bolsheviks staked their future on a revolution there, the German workers let them down. Nor did Moscow's later efforts to undermine the Weimar Republic result in the expected collapse of capitalism—only in the emergence of the mortal threat of Nazism. And when Stalin finally thought he had contained that threat by convincing Hitler of the value of his friendship, this miscalculation nearly cost the Soviet regime its existence in 1941. With such dismal experience there is little wonder the dictator afterwards hesitated to commit himself to any German policy. Instead, he saw in the creation of a Polish buffer state the answer to his security problem.

The story of Moscow's wartime policy—or the

lack of it—toward Germany is told in considerable detail in this book by a Frankfurt historian. Extensively using East German publications bearing on the Russians' relationship with the Ulbricht group, Alexander Fischer describes the ignorance *cum* wishful thinking that accounted for the Soviet fumbling, especially in 1941-42. Again the hard-pressed "Fatherland of Socialism" called upon the German workers to revolt—with predictable results. And Moscow's flirtations with the possibility of an accommodation with Germany after Stalin-grad—on which Fischer has up-to-date information—were hardly suggestive of any better sense of reality.

The "Platform of the Bloc of Militant Democracy," prepared secretly by a task force of the Communist exiles during 1944, rightly assumes a prominent place in the book. For this was, as far as we know, Moscow's only master plan for Germany. It envisaged a "National Front" regime that, emerging from resistance committees to be created before the war's end, would rule the whole nation. The upper classes would be deprived of political influence, the economy socialized, and the people educated in love for the Soviet Union.

Yet we might ask just what role this document played in Russian postwar planning. It is remarkable that while its authors all but ignored the prospect of the country's occupation by the Soviet Union and the West, the Russians were busy discussing this very arrangement behind the closed doors of the European Advisory Commission. And it is significant that once the arrangement had received final approval at the time of Yalta, the "Militant Democracy" program was discarded as having been "overcome by events."

Moscow's tergiversations in the last three months of the war are well enough documented in the book, though not nearly so thoroughly explained. Thus the old question of whether the Russians really favored the dismemberment of Germany, and, if so, why they apparently reversed themselves in the spring of 1945, remains unanswered. More attention to the chronology of the Soviet military and diplomatic moves, intertwined with those by the Ulbricht group, is needed to understand why—in the words of Colonel Tiulpanov, the political advisor of the Russian military government—Moscow began its rule in Germany "with no ready 'theory of occupation administration'."

It is perhaps understandable that Fischer, a German historian, dwells on how the subsequent Soviet policies precipitated the partition of his country and the Cold War as well. This is fair enough, but does not this admirably researched monograph warrant a less-conventional conclusion? It is a pity that its author has not been



inclined to ask more consequential questions inherent in his fascinating subject matter.

From the Russian point of view, the balance sheet of Stalin's policy ultimately turned out as ambivalent as the policy itself. It is true that the Soviet Union achieved security from another German attack—Stalin's major preoccupation—and even managed to create a satellite German state in due course. Yet this was not really the result of the wartime planning that, for one thing, had not aimed at a partition. Nor was it in the cards that the other part of the divided nation should evolve into the main bulwark against Moscow's hegemonial designs in Europe. Thus it happened that, despite Stalin's hedging, Germany again emerged as a critical testing ground of Soviet aspirations.

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E. STUART KIRBY. *Russian Studies of China: Progress and Problems of Soviet Sinology*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. xiv, 209. \$20.00.

For anyone who has recently followed Soviet studies on China this book will be a great disappointment and for anyone seeking an introduction to the principal facts on the subject it will be a confusing jumble. While the flyleaf proclaims the book as the "first comprehensive account of Russian studies on China," the author himself in the introduction describes it as a "first workbook" on the subject. Actually it is neither. In reality it is an extensive paraphrasing of the proceedings of the All-Union conference of Sinologists held in late November 1971, under the auspices of the newly-organized Far Eastern Institute of the Academy of Sciences.

In twelve rather short chapters (averaging fourteen pages each) the author discusses: the history of Russian and Soviet sinology; ancient, medieval and contemporary China; foreign and domestic Chinese affairs; Chinese ideology, culture, politics and economy; Soviet and world sinology. A bibliography of 547 items is appended at the end. Although the author has attempted far too much and fallen considerably short of the promise contained in the title, several points emerge clearly. The major one is that all Soviet "academic" work on the subject of the People's Republic of China has undergone fundamental change as a result of the 1971 conference. If the general Soviet view earlier was of a China temporarily nudged onto an erroneous path because of Mao Tse-tung's pernicious influence, the new position is of China as an implacable enemy.

The conference placed new political requirements before the Soviet scholar. The three princi-

pal charges are, first, that Soviet scholars must above all else seek to prove through their research the "correctness" of the Soviet Union's historical role in relations with China. Secondly, they must condemn outright the Chinese, and especially Mao Tse-tung's, "deviation," from the true path of "international socialism" and concepts of "Marxism-Leninism." Finally, Soviet scholars are charged with the task of assuming the leading position in worldwide sinology (!)—a task which suggests a major propaganda effort to impose Soviet interpretations of China on the West.

It would have been more useful to have translated and published the proceedings of the conference thus allowing a full and undistorted presentation of Soviet views and interpretations rather than to print this study. It is difficult for the reader to discern where Soviet interpretations end and the author's analysis begins. The result is a book which is not at all helpful to the specialist and quite misleading for the general reader.

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## NEAR EAST

JAMES MELLAART. *The Neolithic of the Near East*. (The World of Archaeology.) New York: Charles Scribner's Sons. 1976. Pp. 300. Cloth \$14.95, paper \$4.95.

Agriculture is generally supposed to have begun in the Near East at least ten thousand years ago and to have laid the foundations for the world's first food-producing and urban societies. The significance of this "Neolithic revolution" for universal history is obvious, and a good book describing the patterns and processes involved should be of great interest to scholars in many disciplines. James Mellaart, a well-known British excavator, has here produced a valuable work that, in spite of certain limitations, is the best synthesis available in English today. The most important sites in the region from the southern Balkans to the Soviet Transcaspiian lowlands are treated in considerable detail, with good summaries of stratigraphies, subsistence, artifacts, and architecture. There are abundant illustrations, an excellent bibliography, a useful list of radiocarbon dates, and several invaluable chronological tables to guide the reader.

Since this review appears in a historical journal, it is fair to mention certain idiosyncratic features as well. Mellaart favors a historical rather than an anthropological perspective toward archeological interpretation, and he sets himself squarely in the humanistic rather than the scientific tradition (insofar as one can make such a distinction). His approach is basically subjective, with a certain

disdain for what he terms grandiose theorizing. Obviously he finds unpalatable the recent attempts by some prehistorians to introduce more scientific theory and methodology into their analyses. For Mellaart, the individual man and his struggles, particularly as exemplified in elite groups, are at the heart of cultural change, rather than such impersonal forces as economic, demographic, or ecologic factors. (Historians will of course find the debate familiar.)

In his introduction and conclusion Mellaart nails his colors to the mast, as far as his own explanatory predilections are concerned. It is these chapters and their unorthodoxies that will probably provoke the most biting criticism of his book. While I disagree strongly with many of Mellaart's views and "explanations"—for example, his argument for the origin of food production in what he rather strangely calls "cities"—this does not bother me excessively. Sandwiched between these two chapters is a great deal of first-rate material. It would be a pity if a rigidly Manichaean outlook by some readers blinded them to the value of this compendium as a teaching and reference tool.

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ROBERT W. OLSON. *The Siege of Mosul and Ottoman-Persian Relations, 1718-1743: A Study of Rebellion in the Capital and War in the Provinces of the Ottoman Empire.* (Uralic and Altaic Series, number 124.) Bloomington: Indiana University Research Center for the Language Sciences. 1975. Pp. xiv, 237. \$16.00.

This is a study of a significant and critical period in Ottoman-Persian relations and, simultaneously, of decades of intense domestic turmoil in the Ottoman capital. To the uneducated reader the title might appear inappropriate for the range of topics Robert W. Olson covers. He gives much space to Turkish socioeconomic conditions and the unrests they engendered. Aggression from the east and unrest at home were not always related, as the study seems to insinuate. Yet the coincidence of timing—to wit, Nader Shah's assaults and such serious uprisings as Patrona Halil's—appear to add a dramatic dimension to the period under review.

The Ottomans met challenges from within and without at considerable costs, not the least of which was social deterioration. A realistic assessment of the results of Nader's final assault on Mosul in 1743 does not seem to merit the range of conclusions Olson reaches. The real hero emerges in a few pages of narration: the *vali* who rallied a disparate population to resist and succeed against an invader in spite of overwhelming odds and a

lack of material support from the hapless and deteriorating Ottoman state.

As meritorious as this study might appear to the student of international relations, it will seem even more so to the Ottoman scholar because of the socioeconomic data that Olson so richly enlists in the chapters dealing with internal conditions. Many of this data are the result of the efforts of the finest Turkish scholars on the subject. Unfortunately it was somewhat difficult at times to perceive the immediate interconnection between such data and the events on the eastern frontier. One of Nader's apparent obsessions was to compel acceptance of the Ja'farī "school" of religious law among the Sunni Turks—a matter they never took seriously in any peace effort.

Accompanied by a useful glossary of technical terms and an extensive bibliography, this study still bears heavy traces of Olson's dissertation. Someone who has had to confront the need to transliterate at once from Persian, Ottoman Turkish, and Arabic must necessarily find himself in sympathy with the author, inasmuch as the language purist cannot be pleased. Grounds of criticism exist when mistranslations are allowed to result in mistranslations, most conspicuous in the footnotes. The author might have avoided this had he not stressed, at times quite unnecessarily, the reproduction of the original with its purported English equivalence. And with so much reliance on terminological and statistical reproductions, one cannot envy the editor his task, for here again it is well nigh impossible to eliminate all typographical and printing errors.

These observations notwithstanding, the author deserves the praise of Western Ottomanists for his painstaking research and for placing in perspective an important period in Ottoman history.

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SADIK A. ASSAAD. *The Reign of al-Hākim Bi Amr Allah (386/996-411/1021): A Political Study.* Beirut: Arab Institute for Research and Publishing; distributed by International Scholarly Book Services, Portland, Ore. 1974. Pp. 209. \$8.00.

The career of al-Hākim, who ruled over the Fātimid Caliphate at the time of its greatest territorial expansion, has attracted the attention of Near Eastern chroniclers, both contemporaries and modern historians of the Islamic Middle Ages. The personality of al-Hākim and the story of his rule that emerge from these writings can only be described as psychopathically complex. Whatever his posi-

tive political, diplomatic, educational, social, religious, economic, and administrative contributions, set forth here by Sadik A. Assaad, may have been, al-Hākim's truly monstrous record of tyranny and horrendous cruelty dwarfs these accomplishments.

Unless some startling new sources are discovered to invalidate either the positive or the negative aspects of his life, our perception of al-Hākim will remain limited by contradictions difficult to resolve even by resorting to methodological sophistry, selective manipulation of sources, or the shifting of historical emphasis. Disappointingly enough, Assaad does not adduce new evidence, and he fails in his attempt at a vindication of the enigmatic caliph. Besides the tenuity of Assaad's argument, his account contains many historical inaccuracies, factual contradictions, onomastic mistakes, inconsistent and bizarre transliteration, and innumerable typographical errors. For example, Assaad wrongly asserts that al-Anṭākī, one of the most important contemporary authorities on al-Hākim, did not reside in Egypt (p. 15); the date of the destruction of the Church of the Holy Sepulchre is given as A.D. 1009 on page 97, but as A.D. 1010 on page 148; the frequently mentioned name of Caliph al-Muʿizz is spelled correctly only once (2n., p. 173); al-Fayyūm is stated to be "a fertile area to the north of Cairo" (2n., p. 143); Barqa consistently appears as Burqa; Mūṣil on pages 111 and 112 becomes Mūsul on page 147; a page from the list of abbreviations, with twenty-three entries, contains fourteen mistakes. Even the date appearing on the jacket is wrong, as is the name of al-Hākim's successor.

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R. N. FRYE, editor. *The Cambridge History of Iran*. Volume 4, *The Period from the Arab Invasion to the Saljuqs*. New York: Cambridge University Press, 1975. Pp. xiii, 733. \$29.50.

The four centuries covered by this volume represent the incubation period of the Iran we know today. In their course, we observe Iran's transformation from a Zoroastrian to an Islamic land. But, unlike the former Byzantine provinces whose link with the past was severed by the Arab invasion, Iran emerges at the end of this period with a distinct identity of its own, the roots of which reach into its imperial past, Sāsānian and beyond.

The Arab conquerors accomplished what no Sāsānian or Parthian shah could achieve: consolidation of all the Iranian peoples of the plateau and Central Asia under one rule. We possess plenty of reliable sources to trace the military and adminis-

trative aspects of the conquest. But much study and investigation remain to be undertaken until we can fully understand why and how things happened the way they did. Iran had shown itself impervious to Hellenism, certain Hellenistic manners and mannerisms at the Arsacid court notwithstanding; yet it embraced, wholeheartedly and eagerly, the alien religion and culture of primitive Islam, brought by a people they had regarded, in a not very distant past, as uncouth vassals. How was the rigid class system of the former Sāsānian state reconciled with the egalitarian principles and tribal organization of the new overlords? Which were the specifically Persian points of social tension, other than the obvious one between Arabs and *mawālī*, which existed wherever the armies of Islam conquered and stayed? Amoretti offers especially valuable insights in this respect in his brilliant analysis of sectarian and heretical movements (ch. 15). While in due time Arabic superseded almost all languages in the conquered territories, the inexpressive Persian vernacular, *Darī*, survived and became the hybrid language of the new Persian. It would see its finest hour at about the same time the prestige language, Arabic, showed signs of losing itself in arcane verbal acrobatics.

Persian genius was quick to assert itself under the new order. Perhaps more than any other people, the Persians helped to make Islam a universal religion and written Arabic a marvelously supple and precise vehicle of expression. Persians also supplied the administrative know-how, at first in a vast and complex empire and later for successive Turkish and Mongol invaders. All six compilers of the canonical *ḥadīth* collections were Persians, and it is to Persians that Islam owes some of its most profound and lasting contributions in both the traditional and foreign sciences. Many of the finest stylists in Arabic were Persians, and Persians distinguished themselves in Arabic grammar and lexicography. Arabic was not "their language"—they had to learn it the hard way—so they were more rule-conscious than the "native speaker." This is why al-Muqaddasī can praise the "correct Arabic" that he heard spoken in *Khurāsān*. In paying tribute to Iran's contribution to Islamic culture and learning, one can easily slip into a kind of neo-Shuʿūbī one-upmanship, as, for instance, when al-Masʿūdī (p. 403) and Ibn Durayd (p. 406), both Arabs of sterling descent and only short-time residents on Iranian soil, are claimed as Persians. Also, a faint anti-Arab bias can be discerned in the Persianized rendition of genuinely Arab names in chapter 1.

Leading experts in the field have contributed the twenty essays of this volume. Of the seven chapters

dealing mostly with political history, the ones on the ʿAbbāsīd period, on the Sāmānids, and on the Būyids are written with that admirable blend of narration and interpretation that raises historiography above the dry account of sequential events. Confronted with a bewildering abundance of names and dates, the reader will probably wish for more than the two dynastic tables given in chapters 4 and 7, especially since the index is incomplete. It is not clear why this volume deals with ʿUmar-i Khayyām, who obviously belongs to the subsequent period of Iranian history.

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ARNOLD KRAMMER. *The Forgotten Friendship: Israel and the Soviet Bloc, 1947-53*. Urbana: University of Illinois Press. 1974. Pp. x, 224. \$10.00.

Deftly and lucidly Arnold Kramer describes how a stunned world heard on May 14, 1947 the USSR's Andrei Gromyko agonizing over "Jewish suffering," as he affirmed "Jewish historical claims" and warned against "any unilateral Arab solution of the Palestine problem." Why the sudden sympathy from a country long known for its anti-Semitism? Kramer points out that the Kremlin decided to support the re-creation of a Jewish state for several reasons: to gain a foothold in the "heart" of the Middle East; to provide access to a year-round warm-water port; to help protect the USSR's flank along Iran and Turkey, dangerously close to oil fields and metallurgical industries; to exploit the once-in-a-lifetime opportunity to fill a vacuum soon to be created by Great Britain; to secure the support of the mostly East European leadership in the Jewish population of Palestine, especially the Communist and the Socialist elements in the East-West struggle; and, to introduce a potent dissident ingredient in the more or less solid Moslem bloc.

When Israel materialized in 1948, Russia was concerned with her survival. Enter Czechoslovakia. Forced by the Kremlin to reject the Marshall Plan, Czechoslovakia saw her economic problems aggravated. The Israelis in the meantime, with the tacit support of the USSR, embarked on fantastic schemes of arms acquisition in the best and worst tradition of cloak-and-dagger exploits. One of the main supply sources was Czechoslovakia. But Russia's flirtations with Israel did not work: Israel emerged as a noisy democracy, much stronger than the Kremlin had anticipated; Israel's East European leadership looked to the West not to the East; the Communist parties in the country showed up poorly in elections; Soviet Jews, thought to be assimilated and

on their way to religio-nationalist extinction, perked up at the sound of Israel's shofar, embarrassing the Kremlin's leadership and causing it to vent its latent anti-Semitism.

Kramer has assembled an overwhelming mass of material—primary, secondary and even tertiary. Many items in the bibliography fall into the latter category. Some of the personal interviews with Israeli underground operatives abroad have appeared elsewhere in one form or another as autobiographies, biographies, or articles. The durable quality of the book, however, comes from the skillful use and the masterful arrangement of data. We have here an authoritative narrative of a remarkable international experience.

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## AFRICA

PETER DUIGNAN and L. H. GANN, editors. *Colonialism in Africa, 1870-1960*. Volume 4, *The Economics of Colonialism*. (Hoover Institution Publication, number 127.) New York: Cambridge University Press. 1975. Pp. xiii, 719. \$42.50.

In this, the last of the volumes in the Colonialism in Africa series, Peter Duignan and Lewis Gann have invited their contributors to answer again that old question of academic debate: was empire a form of economic exploitation? Without first constructing an elaborate conceptual apparatus, most of the contributors have gone directly to the field and have provided answers. The attention to detail concerning trade practices, agricultural operations, and investment patterns makes this volume a valuable compendium of commercial and statistical information as well as a sound historical assessment that will intellectually please all except those individuals rigidly adhering to a Neo-Marxist interpretation.

The tone of the volume is mildly revisionist, with conclusions of an eclectic rather than a doctrinaire nature. The authors generally agree that imperial enterprise stimulated more than it delayed African economic development and was to the benefit of the African peoples as well as to particular segments of the European economies. But the authors also see the process as disruptive, a characteristic of that socioeconomic phenomenon commonly called "modernization." The editors provide the underlying thesis of the many chapters in one of their concluding remarks: "The colonial period . . . forced Africa into the world economy; colonialism supplied the engine for progress and modernization."

With such a general theme, it would be nearly impossible for a multiauthor volume to be con-



sistently outstanding or smoothly integrated. Aside from Gann's essay on the German economic effort, which is perhaps the best brief analysis found on the subject anywhere, the strength of the historical analysis is on the British side. The nationally defined assessments of the French, Portuguese, and Belgian colonial economic policies and practices (found in part 2) seem rather perfunctory. The essays on the French and Portuguese read rather like encapsulated statements of what has been frequently stated before.

Part 3, "Economic Mainsprings," is the best part of the book and one that will be appreciated by student and scholar alike. Here is a balanced set of chapters on the various economic sectors of nineteenth- and twentieth-century Africa with the sort of comparative analysis that enhances the reader's appreciation of the complicated process of colonial economic development. Gerald M. Meier's "External Trade and Internal Development" is outstanding. A crisp review of currently contending economic theories followed by an analysis of the West African cocoa industry, the essay convincingly asserts that the soundest approach to evaluation of development through trade is the eclectic one. Simon E. Katzenellenbogen's chapter "The Miner's Frontier" is also an excellent, detailed description of this important colonial activity and an essay richly supported by statistical data.

The least satisfactory part of the book is part 4, "Social Implications." Most of the essays here are far more restricted than their chapter titles imply. Aside from the brief chapter by Colin Newbury, "Historical Aspects of Manpower and Migration in Africa South of the Sahara," a chapter that bears the Newbury hallmarks of meticulous scholarship and fine synthesis, the other chapters are narrowly focused. The discussion of elites concentrates on the Yoruba, and the chapter entitled "How People Came To Live in Towns" is really a brief review of the growth of Nairobi and Kampala. The chapter on nonindigenous ethnic minorities has the appearance of a general outline with short sections on the roles of education, motivation, and bureaucracy primarily in South and East Africa. In none of these chapters does institutional change in the Belgian Congo or French West Africa figure significantly. In sum, the grander synthesis, characteristic of the rest of the book and one of its most commendable qualities, is missing in these essential chapters. Absent, therefore, is something of the purpose of the section.

With the publication of this volume Duignan and Gann offer the general reader a good introduction to the basic problems of African economic history during the colonial period. And, it must be added, their own prefatory chapter is an

excellent overview of this difficult historical landscape.

Of the volumes in the now-completed series I think the one reviewed here is the most original and therefore the most useful. (I except the remarkable bibliographical volume that is fifth in the series.) In a way, the fourth volume is a balance sheet of empire that will bear close scrutiny but, no doubt, will engender some controversy. However, with overseas colonialism now a historical consideration, not a matter of current foreign policy, polemics on the subject are subsiding. *The Economics of Colonialism* is proof of this fact, as it is evidence of the high quality of analysis that can be brought to bear on a subject for which research materials are not easily obtained or managed.

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ROBIN HALLETT. *Africa since 1875: A Modern History*. (The University of Michigan History of the Modern World.) Ann Arbor: University of Michigan Press. 1974. Pp. xi, 807, lxx. \$15.00.

Robin Hallett, a former research officer at the Institute of Commonwealth Studies at Oxford who is now lecturing at the University of Cape Town, knows Africa and African history. His two volumes, *Africa to 1875* and *Africa since 1875*, represent the best detailed history of Africa written to date. For the first time we have a full-scale history of the entire African continent.

Hallett writes well and his organization is superb. In contrast to Robert Rotberg's dull, pedantic, and narrow *A Political History of Tropical Africa* or Norman Bennett's superficial and naive *Africa and Europe*, Hallett gives us a lively, mature account, and on a grand scale (there are 1,339 pages in the two volumes). This volume begins in 1875 when the vast majority of African people lived in traditional ways and were relatively isolated from the world (Europeans, except in South Africa, were confined to small coastal enclaves). Within twenty-five years the continent was partitioned and the colonial administrations were in place. Within the life span of one person the colonialists would all be gone, except from Southern Africa. By 1975 even the Portuguese had departed; only Rhodesia and South Africa remained as symbols of European colonialism.

This remarkable story is told with verve and a wealth of vivid detail. Hallett divides his narrative into eight regional stories, treating each area chronologically. In the introduction he sets the stage for his account, and in his last chapter he analyzes the process of change in modern African history. There are ten maps and a superb, 59-page, double-columned index. The only things missing and



sorely needed are pictures and illustrations of the land and the people, the lives and livelihoods of those who inhabit this incredibly diverse and large continent.

At his best when describing the processes of change in African history, Hallett organizes his material around certain themes: the erosion of African isolation, the accelerated change and violence brought by the new rulers, and the creative achievements of European colonialism.

One still wonders why the Europeans rushed to acquire some parts of Africa. In West Africa, French *officiers soudanais* spent much blood and treasure to acquire for France what Lord Salisbury (and agriculturists) "would call 'very light land'." Yet British and French troops nearly clashed over the right to control such barren spots as Borgu, and Britain almost went to war with France over who should occupy Bahr al Ghazal in the Sudan.

The early days brought much violence—raids, "small wars," punitive expeditions, village burnings to collect taxes or to get labor. African material well-being only began to improve around 1910. But the new rulers eventually brought peace, new techniques, and new crops. Iron Age African societies were propelled into the age of steam power, the internal combustion engine, and modern medicine. Christianity and schools, cash crops, and wage labor brought accelerated change. Old customs and values fell into disuse, but new ones took their place. The new men looked to the school, to the government office, and to the town and the market for advancement. Together with the white rulers they created a new Africa tied to world markets and dependent on a colonial-built infrastructure of ports, roads, railways, export crops, and mining. For Hallett these creative achievements transcended the damage done by violence. Africa was transformed within a hundred years. The successor states, themselves the creation of imperialists, could only follow in the paths laid down by their recent rulers.

In a book so large and covering so much, there are inevitably errors of fact and interpretation, but these do not mar Hallett's accomplishment. Africa has at last found its historian; this is a textbook of magisterial quality.

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L. CARL BROWN. *The Tunisia of Ahmad Bey, 1837-1855*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1975. Pp. xviii, 409. \$20.00.

The transformation of third world nations into modern states is a subject of continuing interest to historians as well as political scientists. Indeed, the

considerable attempts to document and guide modernization efforts reflect the pragmatic and instrumentalist aspect of much of this concern. From this perspective it is hardly surprising that the historical precedents of many recent modernization efforts are often ignored or casually overlooked in favor of what appear to be contemporary innovations. Yet the obvious risks of facile interpretations or even distortions that result ought to be sufficient incentive to those seeking something more than what "problem-solving" accounts may provide.

One of the virtues of L. Carl Brown's study is precisely its careful exploration of the mid-nineteenth-century origins of Tunisia's experience with reforms of a type associated with modernization. Brown's study, however, is more than an account of the reformist ambitions of the Husaynid prince. It is a detailed description, along traditional lines, of the nature and functioning of the complex system that constituted the governing apparatus of the state on its various levels. This alone makes it a valuable source for students interested in the internal administrative organization of the Tunisian state prior to its confrontation with French colonialism.

Functioning within the hierarchically organized caste of this administrative system, Ahmad Bey and his select entourage of influential aides sought to pursue a policy of what Brown has termed "collaborationist modernization." By doing so they underlined the intimate relationship existing between Tunisia's internal reform program and its international situation. The limitation this juxtaposition of domestic policy and foreign interests imposed is not new. In Ahmad Bey's time it was found operating in the reigns of Sultan Mahmoud as well as Muhammad Ali, both of whose experiences have received far greater coverage than have those of Ahmad Bey. The three cases, with their striking similarities of incentives, limited achievements, and respective impediments to change, are comparable. Nevertheless, the specificity of the Tunisian experience, hitherto generally unattended by historians of North Africa and the Middle East, deserves the attention it receives here if only to provide points of contrast with these more familiar periods of recent history.

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ELIZABETH ISICHEL. *A History of the Igbo People*. New York: St. Martin's Press. 1976. Pp. xiv, 303. \$18.95.

The period of the 1970s is witnessing what might rightly be called an Igbo literary renaissance, but still only a few published works deal directly with Igbo history. The book under review, by an Aus-

tralian married to an Igbo doctor, is indeed the only published historical study encompassing pre-colonial and postindependent Igbo history. Elizabeth Isichei certainly ought to be congratulated for providing teachers, students, and the general reader with needed material. No longer will it be fashionable to say that the Igbos have no history; the reality of that history is now fast unfolding, and Isichei has carved for herself an important niche in Igbo historiography.

The basic weakness of the book lies in the fact that it is inordinately ambitious. It covers the Igbo from antiquity to the present—a task even those more familiar with Igbo life and history would tackle with great trepidation. And since Igbo history is still in its infancy, such a broad focus inevitably lends itself to shabby treatment and irritating superficiality.

Isichei's treatment of Igbo origins and migrations, for example, is disappointing. The author confidently asserts that "this whole book is an attempt to answer the question" of Igbo origins, yet she leaves the question unanswered and does not even discuss sufficiently the various theories of Igbo origins currently in vogue. And how does one reconcile Isichei's hypothesis that "the first human inhabitants of Igboland must have come from areas north—possibly from the Niger confluence" and the purported claims of an Mbaïse elder: "We did not come from anywhere and anyone who tells you we came from anywhere is a liar" (p. 3). Yet Isichei accepts the latter as embodying "an essential historical truth." Isichei apparently does not appreciate the fact that many people do not wish to think of their ancestors having ever lived elsewhere. While on this problem, it should be pointed out that the author appears to have carried out the barest minimum of fieldwork. She relies rather, on the collected works of undergraduates at Nsukka.

This book is far from being a conventional historical monograph. Instead it is a book that touches on diverse topics and themes of Igbo history without any satisfactory exegesis. While Isichei seems to subscribe to the Rodney thesis of "social oppression" in respect to the African slave trade, she does not explore the dynamics of the slave trade in the context of Igbo history. How was it, for instance, that the Igbos constituted "the majority of slaves" in the late eighteenth and nineteenth centuries?

Finally, Isichei's assertion that Chief Idigo of Aguleri embraced Christianity because "he was alienated from traditional religion" (p. 164) is tenuous; her oblique acceptance of the obsolete "detribalized" African (p. 169) is unacceptable; the establishment of the Catholic mission at Emekuku was not at the initiative of local women. These criticisms, however, are not intended to

detract from the value of Isichei's praiseworthy effort. The numerous maps enhance the value of the book, especially for those not familiar with Igboland.

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DENISE BOUCHE. *L'enseignement dans les territoires français de l'Afrique occidentale de 1817 à 1920: Mission civilisatrice ou formation d'une élite?* In two volumes. Lille: Université Lille III; distributed by Librairie Honoré Champion, Paris. 1975. Pp. 947.

In a time when old style imperialism has become a favorite whipping boy, it is pleasant to find a fair-minded, critical study of one aspect of the French mission in West Africa. Denise Bouche emphasizes that the schools reflected directly the society that created them. "In Africa French education was a continuous adaptation of the colonial situation" (vol. 2, p. 895). Some eight hundred pages earlier in her introduction she writes of "immense disillusion" suffered in 1972 by the newly independent states after twelve years of "development." These states timidly returned to some of the "abhorrent" colonial practices such as the *jardin scolaire*, the training of children through the cultivation of land.

What she is saying, if I understand correctly, is that training human beings, particularly those of an alien culture, is a complex, even hazardous undertaking. France tried to educate an elite and simultaneously to teach French to a few villagers who scarcely used it. Never was a serious effort made to provide a general education to what became fourteen million persons. Such a goal the mother country neither chose nor afforded. Instead, one percent of the West African *sujets*, not *citoyens*, received some education, consuming in the process approximately five percent of the area's budget.

This labored Paris doctoral dissertation further illustrates that despite all failures and slanders some deeply dedicated French in West Africa grappled with the educational process. Much cultural imperialism was enforced; Islam, as usual, held firm. In 1960 four former students of the William Ponty school in Gorée—Félix Houphouët-Boigny, Mamadou Dia, Hubert Maga, and Modibo Keita—were heads of state. President Leopold Senghor of Senegal, born in the coastal village of Joal, attended "*le cours secondaire*" in Dakar before his training in Paris.

This is a solid study based upon a wealth of manuscript material, carefully organized, and clearly presented—qualities we have come to expect from students of Henri Brunschwig.

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K. DAVID PATTERSON. *The Northern Gabon Coast to 1875*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1975. Pp. ix, 167. \$13.25.

Several of the present thirty-nine works in the Oxford Studies in African Affairs series have dealt with the general theme of precolonial Western African history. Many have followed a pattern established by the pioneering work of K. O. Dike, *Trade and Politics in the Niger Delta, 1830-1885* (1956), of focusing on African participation in international trade and its consequences for local affairs. Despite the lack of specific clues in the title, *The Northern Gabon Coast to 1875* is very much within this tradition.

The theme of this work is the rise and decline of two small coastal groups, the Mpongwe and the Orungu, as middlemen in the transatlantic trade. It derives mainly from the author's 1971 dissertation, which covers the period 1815-75. In addition to the nineteenth-century material there are two introductory chapters and a brief summary. Throughout the book the author makes good use of archival and secondary sources, which have not before been made available in English. Patterson handles the nineteenth-century factual detail well, but that for the two hundred years preceding 1800 is inadequately treated, making the first two chapters difficult to follow. He includes anthropological data in chapter 1, but he does not integrate it into the chronology. He considers geographical factors, too, but the two undated maps, most helpful for later chapters, are less so for the beginning. The brief summary, which incorporates some comparative observations, deserves removal from last place. Patterson should have amplified it and included it in the major narrative chapters. In particular, the question of the contrasting political development of the two polities deserves more analysis.

There are no startling conclusions here, but there is careful detailing of the external affairs of two related trading peoples from their apogee as merchant monopolists to their precipitous decline into economic dependency before the "scramble for Africa." This work shows conclusively that for the Mpongwe and Orungu, as for other West African middlemen, formal colonialism came as a postscript to an unsuccessful struggle to maintain control over the lucrative international trade. Patterson includes much useful detail about the slave and produce trades from this little-studied area; a good bit of material about the participation of the Portuguese islands in coastal trade; and some tantalizing suggestions about the importance of the Atlantic for intra-African commerce and communications.

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R. S. O'FAHEY and J. L. SPAULDING. *Kingdoms of the Sudan*. (Studies in African History, number 9.) London: Methuen and Company; distributed by Barnes and Noble, New York. 1975. Pp. ix, 235. Cloth \$17.50, paper \$11.75.

Scholarly studies of the region known to medieval Arab geographers as *bilād al-sūdān* ("the land of the blacks") are not plentiful. This vast, inhospitable area stretches from the Red Sea coast to the Senegal River below the Sahara and above the rain forests of Central Africa. The authors have melded their Ph.D. dissertations into a competent short history of the Funj kingdom of the Sinnār and the Keira sultanate of Dār Fūr. These flourished roughly between the sixteenth and the nineteenth centuries in the eastern part of this vast region, now contained in the northern and western provinces of the Democratic Republic of the Sudan.

Following the introduction, devoted mainly to the geography of the region, the book is equally divided between the two kingdoms. Part 1, by J. L. Spaulding, traces the establishment of the mysterious Funj sultanate at the beginning of the sixteenth century in an area encompassed by the Gezira and the Upper Nile Valley. The kingdom's reorganization, administration, economic life, and relations to other powers in the area, the last of which was crucial since it dominated one of the main Nile Valley trade routes, are outlined. Spaulding explores and, where the sources allow, analyzes the decline of the sultanate. Part 2, by R. S. O'Fahey, deals with similar topics and themes as they apply to the Keira sultanate in Dār Fūr. The Keira appeared in the seventeenth century in the western Sudan and lasted in attenuated fashion until 1916 when it was conquered by British-led forces.

This structure allows the reader to compare the two kingdoms' development, administrative organizations, politics, economic systems, and reactions to and relations with outside influences and powers. Both were ensconced on the main trade routes in their area. They faced "unregulated" private commercial activity, principally from Egypt, internal and external migration, and the spread of Islam.

P. M. Holt has asserted that three factors predominate in modern Sudanese history. His first factor is the "indigenous tradition"—the product of the intermingling of Arab Muslims with Africans. O'Fahey's and Spaulding's study allows one to explore this decisive encounter of peoples and traditions in some detail. Holt's second factor, the influence of Egypt, can also be better understood from this work. The authors used a variety of sources, Sudanese, European, and Egyptian, and they have begun to clear away some of the ignorance and myth about this process. Their work makes explicit that such informal influences as the

intrusion of various sorts of traders were as important in their effects on the kingdoms as the imperialist designs of the Muhammad Ali dynasty. Holt's third factor, the imposition of European rule in the area by the British, is not relevant, except indirectly. The authors assert that in these two kingdoms "emerged many of the social and religious institutions, and much of the local administrative structure, of the modern Sudan" (p. viii).

Students of Sudanese affairs, historians of modern Africa and Egypt, and those concerned with the spread of modern Islam should read this work. The authors acknowledge modestly that it is "very much a preliminary account" (p. viii). While the narrative is not compelling and there are indeed gaps, it is a useful piece of comparative history.

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R. M. A. VAN ZWANENBERG with ANNE KING. *An Economic History of Kenya and Uganda, 1800-1970*. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. xxiv, 326. \$17.50.

This comprehensive work deserves to be considered as the best available treatment of its subject. It is an excellent introduction for those contemplating scholarly research in the area, as well as a thorough summary of the uneven researches so far undertaken. The latter dictate the preponderance of discussions on twentieth-, rather than nineteenth-century developments. The authors' concern—contrasting Ugandan with Kenyan history—serves to suggest several generalizations with significance well beyond the East African region. (Given the usual connotations of the words "economic" and "social," the book emphasizes rather more of the former than the latter.)

Population, agriculture, crafts, trade, industry, marketing, money and banking, urbanization—these issues are treated in separate chapters yet are successfully woven together to demonstrate a few key analytical themes. There is an extremely interesting chapter on nomadic pastoralism.

The unifying themes of this historical overview derive from a basic question addressed by the authors: why and how did unevenness (inequality) of wealth, income, and power increase steadily across the last two centuries in Uganda and Kenya. They compare precolonial, colonial, and post-independence periods to find the roots of this uneven development. In doing so, they develop the historical interaction of indigenous and imported factors making for unevenness across each chapter topic. The result is a carefully crafted volume.

This study comprises a significant element in the slowly evolving history of modern imperialism. This evolution of written history is itself a fact of

no small historical significance. The world economy produced by modern imperialism in turn has produced a historically unique, worldwide collaboration of historians toward a fully developed understanding of imperialism and its transforming consequences.

Some secondary faults that will strike specialists concern surprising omissions from the bibliography and the downplaying of the colonial authorities' explicit determination to fashion a wage-labor force where none had ever existed. As fuller bibliographies and treatments of the wage-labor issue are available elsewhere, however, this study survives its weaknesses to stand as a valuable contribution.

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KENNETH INGHAM. *The Kingdom of Toro in Uganda*. (Studies in African History, number 10.) London: Methuen; distributed by Barnes and Noble, New York. 1975. Pp. lx, 186. Cloth \$14.50, paper \$8.75.

Written in the tradition of Kenneth Ingham's previous works, *The Making of Modern Uganda* (1957) and *A History of East Africa* (3rd ed., 1965), this little book is a pioneering, but hardly enlightened, effort in the context of the phenomenal developments in African historiography in the last two decades. It is a concise, straightforward account of the development of the Toro monarchy, which the author sees as "the story of the struggle for sovereignty" with "constitutional conflict and political manoeuvre" remaining "at the heart of the narrative" (p. 1). Most people know the story of how Prince Kaboyo founded Toro; how his immediate successors destroyed the kingdom and King Kabalega reincorporated it into the parent kingdom of Bunyoro Kitara; how Captain Lugard—working for the Imperial British East Africa Company—restored Toro with Prince Kasagama as the new ruler; how on the withdrawal of Lugard and his Sudanese garrisons Kabalega once more reoccupied Toro with his famous *abarusura* (national standing army) in a reign of terror that old men in Uganda a decade ago still remembered with horror, while Kasagama sought refuge in the Mountains of the Moon; and how the Liberal government in Britain, finally committed to the destruction of Kabalega, the greatest obstacle to British success in Uganda, restored the kingdom again in 1893. Ingham retells these stories with craft and subtlety.

The rest of the story—also well known—concerns Kasagama's efforts to consolidate his position, extend the kingdom beyond Kaboyo's wildest dreams, create a Toro nationality, and make himself a force to be reckoned with in colonial Uganda



before his death in 1929. The story of Toro between 1929 and President Milton Obote's abolition of it in 1967 is dull, compared with the dramatic developments of earlier years. However, Ingham provides insightful observations. For example, he notes correctly that propped up by "the twin foundations of British support and Bunyoro tradition" (p. 105), Kasagama set out to propagate "the myth of an ancient and fairly extensive kingdom in which the British came to believe almost as fervently as he himself did" (p. 166). Kasagama was a classical collaborator, incessantly humiliated at the game he played with regal determination and consummate aplomb. If Kaboyo founded the Toro kingdom, Kasagama created Batoro—the Toro "people." Ironically, British support of a Toro kingdom gave rise to their narrow provincialism and spurious ideas of ethnicity totally at variance with Britain's professed efforts to create Ugandans as well as Uganda.

If judged in terms of administrative history, concentrating attention "upon government and governors, rather than upon the people as a whole" (p. viii)—and, one may add, upon social and economic developments in history—Ingham is a craftsman with considerable skills. But readers familiar with African historiography of the 1970s will question the value of his approach. Furthermore, because of its narrowness of emphasis, superficial research, and the author's tendency to pay as much attention to neighboring kingdoms as to Toro itself, *The Kingdom of Toro* is a great disappointment. Had Ingham consulted the wealth of materials on Toro history at Makerere University, and had he been systematic in his consultation of the relevant journals, he would probably have written a better and more informative book.

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FRANK HARDIE. *The Abyssinian Crisis*. Hamden, Conn.: Archon Books. 1974. Pp. 294. \$15.00.

Italy's invasion of Ethiopia in October 1935 posed a direct challenge to the League of Nations' system of collective security—a challenge the system did not meet. Mussolini annexed Ethiopia and Hitler invaded the Rhineland in the spring of 1936. Although subsequent events at Munich overshadowed its importance, this first great act of appeasement confirmed Mussolini and Hitler in power, shattered the League of Nations, and served as a stepping-stone to World War II.

Because of these significant events, most serious studies on Ethiopian relations with the outer world have centered on Mussolini's conquest during the

years 1935–36. Using unpublished sources, confidential prints, and cabinet papers now available in the Public Record Office in London, Frank Hardie offers the most recent contribution to the field. In his introductory comments, the author states his thesis—that Britain "bears more responsibility than any other State. For this reason all chronicles of the Abyssinian crisis should revolve round British policy. This is the explicit object of this book." Using this British thread, the author skillfully untangles the knotted factors surrounding the crisis and incisively demonstrates its impact on the rest of the world.

Hardie divides his thoughtful study into three parts. First, he sets the stage with a careful examination of the positions of Abyssinia, Italy, Britain, France, the other great powers, and the League of Nations on the eve of the crisis. He then traces the tragic events from the Walwal incident in December 1934 to the infamous Hoare-Laval Pact twelve months later. He concludes with the question: "For what went wrong, who was to blame?" (p. 232). In placing responsibility for the overthrow of Abyssinia and the League of Nations, Hardie posits some insightful and controversial assessments.

For the most part, this book makes for absorbing reading. A general treatment, rather than a detailed, day-by-day account, it will be welcomed by the student and general reader. The historian interested in the field will make the following observations. Hardie relies heavily on Arnold Toynbee's partisan study in the *Survey of International Affairs*, 1935 (vol. 2, 1936), subtitled "Abyssinia and Italy." George Baer's exhaustive scholarly work, *The Coming of the Italian-Ethiopian War* (1967), which concentrates on diplomatic events, is seldom mentioned. Although Baer did not have access to the unpublished British documents, his excellent study is an accurate account of the ominous events of 1935. Hardie failed to use other important published works. Manfred Funke, relying heavily on unpublished German sources, gives fresh insight into Germany's role in the crisis in *Santionen und Kanonen: Hitler, Mussolini und der internationale Abessinienkonflikt, 1934–1936* (1970). Giorgio Rochat's *Militari e politici nella preparazione della campagna d'Etiopia* (1971), which includes the private papers of DeBono and Badoglio, is a major work on Mussolini's early military preparations for war. Although Hardie refers in his footnotes to the confidential prints (FO 401), a closer examination of series 371 (political) sheds a different light on the role of the papacy, which the British believed to be financing Mussolini's adventure, and on Tafari's role in the 1920s, in particular his lack of any real influence in the formulation of the 1928 Treaty of Friendship and Arbitration. A



careful scrutiny of these sources would add to the total perspective on the Ethiopian crisis and sharpen the focus on Britain's role, thereby strengthening the author's thesis.

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JOSEPH C. MILLER. *Kings and Kinsmen: Early Mbundu States in Angola*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1976. Pp. xviii, 312. \$22.00.

This is the first attempt to synthesize the early history of the Mbundu peoples of Angola. Of nine chapters, only one deals with the period of direct interaction between the Mbundu and the Portuguese; thus, the major part of the book is devoted to indigenous historical developments and to Mbundu relations with their Central African neighbors before the seventeenth century. Joseph C. Miller bases his study on published works, including a wide range of linguistic and anthropological sources as well as historical accounts; on archival materials, especially those in Portugal; and on oral history that he collected during a five-month residence in Mbundu territory. The comprehensive bibliography is particularly useful.

The central theme of the book is the process of state formation: the problem of how, why, and when peoples organized through kinship loyalties that were essentially particularized and localized, how they first developed political institutions that cut across these ties, and how large-scale centralized political structures emerged. In an introductory critique of the historiography of state formation in Africa, Miller discusses the past tendency to explain the development of kingdoms in terms of such external catalysts as migrations, conquests, or the need to organize trade. For the Mbundu, however, Miller emphasizes the internal dynamics of the process. A central issue in the discussion is how the various Kimbundu-speaking peoples attempted to resolve conflicts engendered by their traditional and continuing loyalties to kinsmen and their respect for the newer type of political authority embodied in their kings.

This study should be of interest not only to African historians but to all involved in the reconstruction of the past of preliterate societies. The author includes a discussion of the techniques used to collect oral data and assesses the strengths and weaknesses of his sources. His treatment of the oral traditions and their historical content will provoke discussion. His study of the Mbundu states adds much to our understanding of early Angolan history, raises questions for future re-

search, and provides clues to the wider history of the Central African savanna regions.

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ALAN R. BOOTH. *The United States Experience in South Africa, 1784-1870*. (South African Biographical and Historical Studies, number 22.) Cape Town: A. A. Balkema. 1976. Pp. xi, 236. \$17.50.

Alan Booth's book deals with United States involvement in South Africa from 1784 to 1870, which was characterized by trade, mission stations, and consulates. Trade, in which South African ports served both as markets and revictualing stations, involved the exchange of American flour, tobacco, barrel staves, hops, furniture, lumber, clothes, and other smaller items, for South African wines, hides, skins, wool, and whales. By 1865 the United States was second to Britain in South African trade, accounting for about twenty percent of South Africa's wool exports, thirty-five percent of its hides, and over half of its skins.

Missionary work began in 1835 when the American Board of Commissioners for Foreign Missions sent six evangelists, three of whom established a station among Mzilikazi's Ndebele in the Transvaal, while the others worked among Dingane's Zulu in Natal. The Ndebele mission collapsed in 1837, when the Voortrekkers routed the Ndebele across the Limpopo. The Zulu mission also collapsed in 1840 when Dingane was defeated by the Voortrekkers. It was resumed successfully among the Zulu refugees, who were by then relocated in "native reserves" by Theophilus Shepstone.

Booth also discusses the sociointellectual origins of the involvement. He postulates that post-independence Americans were motivated by the principles of the American Enlightenment (humanism, rationalism, and physiocratic exchange), which sought to achieve world-wide "a more perfect life, on the American Model, and in the name of God." Booth does not, however, establish the relationship between the Enlightenment and the activities of individuals on the spot. One wonders whether the missionaries personified the Enlightenment, when there were racial incidents on the stations and when one of the missionaries, Daniel Lindley, chose to minister to the Voortrekkers "out of personal conviction that the African was beyond salvation" and that the Boer was the key to the African's destiny.

Booth's monograph is a valuable addition to the literature dealing with United States-South African relations. With the exception of the occasional use of such sensitive terms as "native" and "kraal," the book has an African perspective.

Booth is also to be commended for his extensive use of primary sources.

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#### ASIA AND THE EAST

TIMOTEUS POKORA, annotated translation with index by. *Hsin-Lun (New Treatise) and Other Writings by Huan T'an (43 B.C.-28 A.D.)*. Ann Arbor: University of Michigan Center for Chinese Studies. 1975. Pp. 414. \$5.00.

Because of China's long history and the multitude of records of the past, generalizations about the country are both necessary and hazardous. One such generalization may be found in the stereotype of Confucianism. To characterize the Confucian personality and thought as conservative, bookish, antiquarian-minded, authoritarian, and conformist may be correct, but the application of such characteristics to each individual Confucian must be tested in empirical studies. And these studies have produced so many exceptions that scholars may question whether exceptions still prove the rule. Recent examples include Winston Wan Lo's study of Yeh Shih (1150-1223) (1974), my own work on Hsün Yüeh (148-209) (1975), and Timoteus Pokora's second major work on Huan T'an (43 B.C.-28 A.D.).

Born into the family of a "professional" Confucian, Huan T'an lived from the last years of the former Han dynasty, through the turbulent years of the Wang Mang interregnum, into the early part of the later Han dynasty. He remained in his minor official position under five rulers and two dynastic changes, and he survived, not by conforming to the powerful political and dominant intellectual movements of the time, but by resisting such currents and maintaining his political neutrality and independence of mind. At the end, he barely escaped public execution at the court, but died on his way into exile.

As a scholar-official with versatile knowledge of the Confucian classics and a music specialist, Huan T'an deserted the Confucian orthodox "Modern Text School" and joined the unofficial "Ancient Text School" in interpreting the classics; but in spite of the penchant of the "Ancient Text School" for antiquarian studies, he ridiculed antiquarian-mindedness. Although Confucius had specifically denounced the music of Cheng as lewd, Huan T'an defended its modernity. In a time when Confucius was hailed not only as a sage but as a divine prophet, especially in the *ch'an-wei* apocryphal-prognostic works vigorously promoted by the Emperor Kuang-wu of the later Han, Huan

T'an asserted that "Confucius was but an ordinary man" whose *Ch'un-ch'iu* canon (the Spring-and-autumn Annals) was no more valuable than Huan's own *Hsin-lun*. Furthermore, he repeatedly denigrated the *ch'an-wei* works in front of their powerful patron, the Emperor Kuang-wu. In a time of rising religious superstitions in the name of Taoism, Huan T'an proclaimed that body and spirit, life and death, were like "the candle and its flame"—when the one burns out, the other extinguishes. At the highly conservative later Han court, he counselled: "To let the enlightened and the wise plan affairs and then bring them down to the masses will certainly be inadequate." According to Pokora, Huan T'an seemed to hint at the modern Maoist "mass line" (p. xxi), though this interpretation seems a bit far-fetched and out of context. Actually what Huan T'an wrote was a criticism of the mediocre majority of officials who failed to support and follow the advice of the enlightened few, and a protestation that the wise must maintain independence of thinking from the vulgar masses (pp. 16-18).

Pokora began the work in Peking more than twenty years ago. He published "The Life of Huan T'an" in *Archiv Orientalni*, number 31 (1963), 1-79 and 521-76. The present book contains a detailed reconstruction of the *Hsin-lun* and the re-collection of other writings by Huan T'an, translated into good English with annotations.

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JOHN WINTHROP HAEGER, editor. *Crisis and Prosperity in Sung China*. Tucson: University of Arizona Press. 1975. Pp. xv, 264. Cloth \$8.95, paper \$4.95.

This volume contains eight articles delivered at the Sung II Conference held at Munich in 1971 and an introduction by editor John Winthrop Haeger.

Yoshinobu Shiba, in "Urbanization and the Development of Markets in the Lower Yangtze Valley," bases his discussion upon two main themes: population concentration and commercial tax. He pays special attention to the commercial development of Hu-chou and Hui-chou, two typical areas, to illustrate differences in the process of commercialization between plain and mountain regions, and includes fifteen tables. E. A. Kracke, Jr. in "Sung K'ai-feng" presents a comprehensive description of the growth and development of K'ai-feng, the Northern Sung capital, providing extensive documentation with materials from Chinese and Japanese sources. Both articles concern urbanization. During the T'ang-Sung period, it may be noted that quality rather than quantity characterized the trend of urban growth; such a

trend can be seen from the decreasing number of *Hsien* during that period.

Brian E. McKnight's "Fiscal Privileges and the Social Order in Sung China" and Edmund H. Worthy's "Regional Control in the Southern Sung Salt Administration" concentrate on economic history. John W. Haeger's "1126-27: Political Crisis and the Integrity of Culture" and Conrad Schirokauer's "Neo-Confucians Under Attack: The Condemnation of *Wei-hsüeh*" deal with political history. Rolf Trauzettel's "Sung Patriotism as a First Step Toward Chinese Nationalism" and Charles A. Peterson's "First Sung Reactions to the Mongol Invasion of the North, 1211-17" discuss topics in military history.

Some minor mistakes exist; *Kuo-tzu chien* should be translated as the Imperial Academy (p. 91), and "Kuang-tung West" should be changed to "Kuang-nan West" (p. 102). As a whole, however, this is an excellent work on Sung China.

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PETER WARD FAY. *The Opium War, 1840-1842: Barbarians in the Celestial Empire in the Early Part of the Nineteenth Century and the War by Which They Forced Her Gates Ajar*. Chapel Hill: University of North Carolina Press. 1975. Pp. xxi, 406.

Peter Ward Fay treads well-traveled ground in his descriptive narrative of the First Opium War, 1839-1842. Written largely from the British imperial perspective and seen through the eyes of Westerners in China in the 1830s, the book traces the origins and development of Western trade in the Canton system, the conflicts which arose between Chinese and Westerners in the 1830s, and the details of the war itself right up to the last shot fired. The centrality of opium is apparent throughout the work as the chief causal factor of the war from both the Chinese and Western points of view. The main thread of the narrative is found in its thorough-going treatment of the evolution of British policy in China in the 1830s through the war years.

The book's greatest strength lies in its comprehensiveness in treating the Western experience in China in the early nineteenth century and in its penetrating account of the fabric of the Western community in Canton and Macao. Especially intriguing is the account of the Catholic missions which were based in Macao and regularly sent priests far into the hinterland. The comprehensiveness of the work could be onerous if Fay were not such a superb writer, but some may find the lengthy descriptions of ships and troop maneuvers rather heavy going.

The work is rich in detail and made richer still

by the author's forceful and robust writing style which makes it almost possible to see and breathe the opium episode. He paints a historical picture with vivid colors, capturing the imagination of the reader, carrying it with him through the poppy fields of Uttar Pradesh, along the muddy streets of Macao, and into the malaria-infested troop encampments on Chusan. As a result, he succeeds in re-creating the historical moment for the reader and in so doing heightens one's understanding of the whole Opium War episode. However, this historical recreation is seen largely through the eyes of Westerners, which the author readily acknowledges (p. 389), and to some extent this affects the interpretive aspects of the work. Yet, on the whole, Fay's treatment of the Chinese perspective is both sensitive and substantial, revealing a thorough and insightful reading of European monographic literature on the subject.

His work is built on an impressive foundation of primary research in European language materials. He has mercifully provided both maps, which aid the reader in following his descriptions of trading, diplomatic, and military movements, and a very good index which a work of this type requires if it is to be used meaningfully for reference purposes. His vigorous writing style evokes a sense of immediacy about the historical event which is rarely captured in monographic literature, and one feels the book should be classed somewhere between monograph and historical novel. At the same time, by its careful research and painstaking detail, it merits attention as a serious historical work.

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FREDERIC WAKEMAN, JR. *The Fall of Imperial China*. (The Transformation of Modern China Series.) New York: The Free Press. 1975. Pp. xi, 276. \$10.95.

Despite its title, Frederic Wakeman's book is actually a history of China during the Ch'ing period. There are four introductory chapters, the best being the "Gentry," in which Wakeman exhibits some rare insight, especially on the relationship between the higher and the lower gentry. The least satisfactory is the "Dynastic Cycle" as the author deemphasizes demographic and economic realities that formed the backbone of dynastic changes. The remaining seven chapters are more or less standard, tracing the rise and fall of the last Chinese dynasty.

The book is written mostly from secondary sources in English; primary sources in Chinese, including the standard and familiar ones, do not appear in either the footnotes or the bibliography. The fact that a respectable book can be written on

mostly secondary sources indicates how far Western scholarship on Chinese studies has matured, but it also creates some problems. One problem stems from Wakeman's regarding as valid all situations, when monographic or other studies may have established only one of the situations as valid. Realizing that situations in China can differ vastly from one place to another, traditional historians in China, whenever impelled to make a particular point, solve the dilemma by quoting relevant sources without making a sweeping statement, thus letting the reader draw his own conclusion. This, of course, is not the way a Westerner would normally write history.

The book also contains several factual errors. Yeh-lü Ch'u-ts'ai was not a Uigher (p. 65); he was a Khitan who often allied himself with the Chinese gentry against the Uighers (*P'ian shih*, roll 146). The "many varieties of outlaws" (p. 62) were not known as *yu-hsia*, a term used to characterize individual and individualistic bravadoes of a much earlier period; they were known to admiring peasants as *Lü-lin hao-han* during the period that Wakeman covers. "Long hair" was not a traditional symbol of revolt (p. 161); in fact one of the Confucian classics (*Hsiao ching*) mandated it as a normal style. It became a symbol of revolt for the Taipings only because the Manchus had insisted that all loyal Chinese must shave part of their heads. *K'e-t'ou* does not mean "three kneelings and nine prostrations" (p. 123), which is a translation of *san kuei chiu k'ou-shou*. Wakeman also could have helped his readers more by making a distinction between u and ü in his romanization of Chinese characters.

These points are comparatively minor in view of the overall merit of the book. Wakeman knows his subject well, and his writing is good, so good in fact that some of the passages (for example, pp. 216-17) sound almost poetic. It is a fine book for both scholars and laymen.

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ROGER PÉLISSIER. *Les bibliothèques en Chine pendant la première moitié du XX<sup>e</sup> siècle*. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Le monde d'outre-mer passé et présent. Fourth Series, Bibliographies et instruments de travail, number 9.) Paris: Mouton. 1971. Pp. 366. 40 fr.

Library histories usually find few readers even among librarians. Roger Péliissier's study, however, covering the period from 1898 to 1949, provides new insights into the cultural modernization of China and deserves the attention of scholars in this field. The manner in which libraries responded—or

failed to respond—to the great need for information and the hunger for knowledge of the world, of science and technology, and of current events influenced the course of modern Chinese history.

Libraries have existed in China for more than two thousand years, but research and public libraries as they are known today emerged only in the twentieth century. Their inspiration and model was the American library movement, then the most advanced in the world. Once introduced, modern libraries proliferated rapidly, increasing from 522 in 1918 to about 3,000 in 1936, with the greatest concentrations in such traditional intellectual centers as Peking and the Yangtze Basin. Despite idealistic programs aimed at reaching them, the great rural populations remained largely untouched by the library movement. Financial support from American and British organizations underwrote the operation of such distinguished institutions as Boone Library School and the National Library of Peking, but most libraries were beset with financial problems. An insufficient number of trained librarians and technical difficulties related to cataloging Chinese books also hindered their development. Nonetheless, by the 1930s, college and research libraries were flourishing, and small public libraries and reading halls had been established throughout the country. World War II brought this progress to a halt; more than 1,000 libraries were closed or destroyed and others suffered losses from bombardment, pillage, and fire.

Roger Péliissier (1924-72) was the curator of the Chinese Collection at l'Ecole Nationale des Langues Orientales and the associate director of the Centre de Documentation sur l'Extrême Orient. His work, the first monographic study of modern libraries in China, breaks new ground. It was Péliissier's doctoral dissertation, presented at the University of Paris in 1969. Unfortunately, it has been issued in an unrevised format. The author presents a great number of statistical data in more than thirty tables and charts. While most of them are clearly formulated, occasionally the graphics are confusing and the text does not fully interpret the data. The bibliography does not list all the sources available, but it remains useful as a starting point for others who wish to pursue this topic. Péliissier also includes short biographies of thirty-six librarians.

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MICHAEL GASSTER. *China's Struggle to Modernize*. (Studies in World Civilization.) New York: Alfred A. Knopf. 1972. Pp. xiv, 154, vi.

EDWARD J. M. RHOADS. *China's Republican Revolution*:



*The Case of Kwangtung, 1895-1913.* (Harvard East Asian Series, number 81.) Cambridge: Harvard University Press. 1975. Pp. 366. \$18.00.

Both of these studies reflect recent perceptions of China's revolutionary process that have developed from a more careful assessment of the pace of change and the growth of internal cleavages during the last years of Ch'ing rule, which in turn shaped the context of political struggles after 1911.

Michael Gasster's work focuses on intellectual change in the period of the 1911 Revolution, and in this small and elegantly crafted handbook he brings this perspective to bear on the larger problem of "modernization" in the late nineteenth and twentieth centuries. Designed as part of a series of paperbacks entitled *Studies in World Civilization*, this book is the modern counterpart of Frederick Mote's *Intellectual Foundations of China*, and it is intended primarily to serve as a complementary and comparative summary for students of the problem of modernization in Western history. Gasster's book includes a wide-ranging, highly selective bibliography and a useful chronology of events between 1839 and 1971. Like Mote's work, it is self-consciously Sinocentric, at the same time underscoring themes that link China's modern history with world history.

Gasster is interested in ideas and culture and in the role that China's elite—particularly its intellectuals—played in building a modern Chinese nation. His account shows how China's leaders gradually distinguished between the process of Westernization and modernization, and how they then resolved the conflict between the requirements of Westernization and modernization, on one hand, and their own nationalistic commitments to Chinese culture, on the other. He describes the widening of urban-rural differences and of social cleavages as modernization proceeded, and he emphasizes how concepts of mass organization that the Chinese Communist party articulated helped reweave a fragmented social fabric.

Gasster argues that in the end, the Western role "revitalized" rather than transformed Chinese culture. In keeping with this view, his summation is extremely cautious and moderate, emphasizing the staying power of elements of traditional Chinese culture that persist along with the radical changes effected since 1949. He also stresses the experimental nature of the new organizational forms that the CCP initiated in its constant adaptation to distinctively Chinese patterns.

Edward Rhoads' book describes the Revolution in Kwangtung Province from the end of the Sino-Japanese War to the abortive "Second Revolution." His short-range dynamic view is designed to emphasize accelerating and irreversible social and

political change. Rhoads' findings show how certain issues in the years preceding the Revolution served as vehicles for pressing the interests of various elite groups, as well as for defining their commitments to nationalism and anti-imperialism. While in his view commitment to political change grew steadily among urban Chinese after 1895, support swung decisively away from the dynasty and toward revolution only in the final months (November 1910 to May 1911). In those critical months, he argues, specific controversies—ranging from the appointment of a Manchu cabinet to the government's opposition to queue-cutting, and including railway rights and financing—coincided to accelerate the revolutionary process in Kwangtung.

The main strength of the Rhoads study is its emphasis on the multifaceted nature of "revolutionary" leadership; gentry, merchants, overseas Chinese, and charitable associations, as well as members of the new army, the provincial assembly, and the T'ung Meng Hui, all played vital and frequently conflicting roles in changing the distribution of power in Kwangtung Province and in the proclamation of a quasi-federalist republic in 1911.

This strength, however, becomes a weakness in the absence of analytic tools sufficient to guide the reader through thickets of well-documented narrative evidence. The work begins with the promise that a Chalmers Johnsonian model of accelerating revolutionary dysfunctions will inform the study, yet the outlines of the model are not drawn clearly enough through the account that follows. Three conceptual problems stand out. The first is the difficulty of using the province as a unit of analysis when frequently the evidence mustered applies only to a few urban centers. The second is the problem of assessing the pace or rate of political change in a historical perspective. The third is the vague use of the word "politics" and the failure to consistently distinguish the political interests represented by the multiple contending forces in this period.

Rhoads acknowledges (p. 5) all the biases in his evidence: they are "socio-political rather than economic, urban rather than rural, and 'bourgeois' rather than peasant." References to a "province-wide" movement (p. 88) and "the people of Kwangtung" (p. 65) contrast, however, with statements such as "the nationalist movement was essentially an urban phenomenon" (p. 151), and again, "unlike the nationalist movement . . . the self-government movement penetrated into the rural areas" (p. 153), though in some cases such penetration was purely semantic (p. 175). Accounts of the pace of change—very convincing for the critical period mentioned above (see chapter 9)—similarly lack credibility elsewhere. In particular, the description



of a Kwangtung tradition of "political passivity" (pp. 23–28), and of the sudden rapid spread of foreign influence over a brief five-year period after 1895 (pp. 14, 32), does not accord well with our knowledge of local-level militarization in the province; nor does it take account of antimissionary disturbances prior to 1895, or the impact of foreign trade, particularly smuggled opium, on the countryside. The third conceptual difficulty is highlighted on page 179, where Rhoads argues that the "nationalist demonstrations had politicized the merchants; the self-government movement now politicized the gentry." He thus confines his concept of political interests to the narrow sphere of participation in nationalist movements or in official administration. He is much closer to the mark when he analyzes the constitutionalist movement as a governmental program aimed at controlling emerging political interest groups (p. 99). Had he attempted a description of the evolution of such interest groups in a more systematic way—linked to economic change, for example, as well as to social and political change—his work would have assumed more coherence.

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JAMES PINCKNEY HARRISON. *The Long March to Power: A History of the Chinese Communist Party, 1921–72*. (Praeger Library of Chinese Affairs.) New York: Praeger Publishers. 1972. Pp. xvii, 647. \$18.50.

DICK WILSON. *The Long March, 1935: The Epic of Chinese Communism's Survival*. New York: Viking Press. 1972. Pp. xx, 331. \$8.95.

These two books, different in many respects, achieve a felicitous combination of an appreciation for the epoch-making dimensions of the Chinese Communist achievement with a sober respect for historical fact and scholarly objectivity. Though each author refers to the famous Long March in his work's title, only Dick Wilson is solely concerned with this anabasis. James P. Harrison's book is more ambitious, attempting a comprehensive history of the Chinese Communist party from its founding in a Shanghai girls' school in July 1921 to the visit of President Richard Nixon in February 1972—and to an admirable degree he succeeds. His book is long, exhaustively detailed (it is documented with no fewer than 1,746 footnotes drawn from Chinese, Soviet, and Western sources), and conscientiously fair-minded in its interpretations. Indeed, Harrison's book may stand for years to come as the most definitive and balanced summation of the state of our knowledge of the development of the Chinese Communist party.

Although Harrison's strong point is his patient, painstaking accretion of historical detail into a

logically sequenced and readable narrative, his interpretive insights are also valuable. He constructs his account around two "interlocking themes": first, the party's ability to appropriate the "spirit of revolutionary nationalism" from the Kuomintang, and, second, the Communists' success in organizing the masses to wage revolution through the "mass line." Harrison regards these two themes—long juxtaposed as rival theories of the origin of the Chinese Revolution—as wholly complementary. He argues cogently that "nationalism was usually the more popular cause in the more populous Communist areas behind Japanese lines, which furnished the bulk of new recruits, but recruits in the original Shensi base and top party leaders continued to be primarily committed to the social revolution as the only effective way of 'saving the nation'" (p. 272). Though he goes on to provide a useful summary of developments in the former arena in chapter 14, where he describes CCP expansion in the North China wartime base areas, Harrison probably makes his most valuable contribution in his analysis of the implementation of the social revolution. This appears in chapters 15 and 16, dealing respectively with the Yen-an base area and the Cheng-feng rectification movement. Chapter 16 is of particular interest, bringing out numerous unexpected parallels between this earliest party rectification movement and the later Great Proletarian Cultural Revolution; one begins to suspect that the elements of mass spontaneity and shifting leadership alliances which characterized the latter movement are not so atypical of the campaign phenomenon as was previously assumed.

Harrison's vantage point for this narrative is that of the Chinese Communist party elite, and he provides an excellent account of the almost endless series of conferences where this small group gathered to discuss policy alternatives, conduct spirited disputes, reach binding decisions, and reconstrue previous decisions. Using the recently published memoirs of Otto Braun, Chang Kuo-t'ao, Chou Fo-hai, and Madame Chang Kuo-t'ao (as well as all available party documents), Harrison illuminates the important role played by Ch'en Shao-yü in the formation of the Second United Front, the political functions of the Mao-Liu alliance, and the great ideological suppleness of Mao in intraparty disputes (in contrast to his more inflexible public posture). For example, Harrison indicates that Mao was the moderate and Liu Shao-ch'i the "radical" during the land reform campaign immediately prior to the Communist victory in 1949. His description of the evolving organizational structure and the fluctuations in party membership is precise and painstakingly thorough. The emphasis on policy formulation entails a corresponding neglect

of policy implementation, in some cases, but one cannot focus on every aspect of a process at once, and Harrison's priorities seem prudent given the limited availability of information about the latter.

According to Harrison's narrative, the Chinese Communist party then emerged in the context of a political culture of rampant Machiavellianism, in which hypocritical alignments and sudden betrayals were the order of the day. Nor was this by any means unique to the CCP, which may have actually been more sinned against than sinning, if one considers the infamous Shanghai coup d'état of Chiang Kai-shek or the treachery of Sheng Shih-ts'ai or Ku Shun-chang. This was one of those periods of transition and social upheaval that Durkheim termed "anomie," when norms tend to lose their binding effect. From this perspective the CCP's priority on the vigorous enforcement of public virtue seems as natural and understandable as the convoluted factional machinations occasionally manifest during its own intraparty struggles, such as the Cultural Revolution or the Lin Piao affair.

In view of the conspicuous and solid virtues of the book, it seems almost captious to advert to its few lacunae. The two "interlocking themes" that are meant to unify the text have not been integrated well into the narrative, but seem rather to have been added in afterthought. A somewhat more important point is that although the book claims in its subtitle to render the "history of the Chinese Communist party, 1921-72," the first twenty-eight years receive much more careful treatment than the last twenty-three. This is evident not only in the page count (432 pages versus 80) but in the level of analysis. Whereas the post-Revolutionary years have already provided a surfeit of fine secondary literature upon which a more thorough examination of this period might have been based, much of this literature remains controversial, and Harrison may have quite wisely determined to let time provide the necessary detachment for future historians to describe the recent period with the same exhaustive objectivity apparent in his own description of the pre-Liberation period.

Perhaps the archetypal symbolic experience of the Chinese Communist Revolution was the one that brought the movement nearest to utter extinction: the Long March. Dick Wilson's work, surprisingly enough, is the first book-length study of that experience to appear in English, and it is probably the best account yet. Though he generously accredits the earlier classic accounts of Edgar Snow and Agnes Smedley, Wilson's book supersedes them in scholarship and sobriety.

Wilson has provided a fast-paced and gripping account in which he manages to capture both the

historical and the mythical and symbolic aspects of the Long March. He is most severely hampered in his endeavor to reconstruct the factual outlines of the march by the paucity of primary sources; as he explains, the party archives for the march were lost in its course, there is no collection of captured materials or newspaper coverage, and the leaders kept no personal records. He might have utilized the vast KMT compilation on the civil war with the CCP (*A History of Military Action against the Communist Rebellion, 1930-1945*) in order to counterbalance his extensive use of CCP sources. Excepting this minor oversight, his research on the subject is truly encyclopedic—as evinced by more than four hundred footnotes from sources in four languages—which enables him to fill in many of the gaps in previous accounts. Wilson infers that the Red Army was able to break out of the Fifth Encirclement (and perhaps to survive numerous subsequent battles with nominal KMT troops) by exploiting weak links in the KMT chain created by regional and factional animosities. He tentatively concludes that there was indeed a major battle with Kwangsi troops under Pai Ch'ung-hsi at the Hsiang River, and he argues persuasively that Szechwan was the original final destination of the Reds after breaking out from Kiangsi, and that it was Chang Kuo-t'ao's flight across the Chia-ling River in February 1935 which prevented them from reaching it. Through a meticulous examination of all available materials, he manages plausibly to reconstruct the crucial meetings at Tsunyi and Maoerhkai at which Mao Tse-tung emerged as the party's new leader.

To evoke the mythical dimension of the Long March, Wilson relies quite heavily on long quotations from eyewitness accounts published in such collections of memoirs as *Hsing-huo Liao-yuan* and *Hung-ch'i P'iao-p'iao*. Whereas these accounts may sound somewhat melodramatic to the Western reader, they render the experience as its survivors recollect it, and as the leadership presumably wishes it to be interpreted. And it is through such accounts that the experience lives on in the hearts of the Chinese people, conveying its lessons of sacrifice, discipline, unity, self-reliance, and the ability of the human will to prevail over seemingly insuperable adversity.

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YUEH SHENG. *Sun Yat-sen University in Moscow and the Chinese Revolution: A Personal Account*. (International Studies, East Asian Series. Research Publication, number 7.) Lawrence: Center for East Asian Studies, University of Kansas; distributed by Paragon Book Gallery, New York. 1971. Pp. 270. \$8.00.

In this memoir by one of the renowned "28 Bolsheviks," the first such account to appear in any language, Yueh Sheng provides us with an authoritative account of the composition and early activities of this group. He also makes a valuable contribution to our knowledge of the important Sun Yat-sen University in Moscow, augmenting his own reminiscences with sources in Russian, Chinese, and English. Former students of the university have written few accounts of their experiences there, and the archives of that institution in Moscow are closed.

Although the university functioned for only five years (1925-30), it educated numerous future leaders of both the CCP and the KMT, including Teng Hsiao-p'ing, Liu Shao-ch'í (for only one year), Ch'en Po-ta, and Chiang Ching-kuo. The latter at this time became an ardent Trotskyite, sharply repudiating his father (Chiang Kai-shek) following the Shanghai coup of 1927. Some of the pedagogical techniques used in mainland educational institutions after Liberation were modeled on teaching methods first used at Sun Yat-sen University. This institution further contributed to the introduction of Marxist ideas into China by undertaking a monumental translation project, rendering the classics of Marx, Engels, and Lenin *et al.* into Chinese. Of course, the university may have had various unintended repercussions as well: if some of the allegations concerning the brutality of the "28 Bolsheviks" in repressing intraparty dissent are correct, this may have reflected GPU tactics in ending the "lacerating divisions" that arose among student factions in Moscow.

Sheng's richest lode of new information for the Chinese history scholar concerns the Sun Yat-sen University. He conveys the atmosphere and offers convincing vignettes of the famous figures who visited the school: Radek, the brilliant pipe-smoking first rector; a very human Krupskaya, who lectured the students on the problems of making love and revolution; Stalin, who appeared to give a speech but did not stay to answer questions; and many others. The reader learns that the Sixth Congress of the CCP was held in a former landlord's *dacha* named "silvery villa" on the outskirts of Moscow and that Bukharin carried a hunting rifle with him at all times and kept a falcon on his shoulder so that he could leave the conference at the first opportunity to hunt in the nearby forest. Such impressions are often interesting and insightful.

These were times of frequent reverses and bitter conflicts in the party, and the ideological issues then current in both China and the Soviet Union reverberated in the halls of Sun Yat-sen University. Every policy reversal (and there were many) precipitated an attempt to define the ideological

source of the original "error" and to pin blame on a high-ranking perpetrator, in turn setting off a series of struggles at lower echelons concerning who should be purged with these deviants. Thus the reader learns how Trotskyism originated at Sun Yat-sen University, with direct links to Trotsky himself, and was then transmitted back to China by returning students who formed an alliance with the disgruntled Ch'en Tu-hsiu and P'eng Shu-chih. It was the struggle against this Trotskyism that occasioned the formation of the faction later known as the "28 Bolsheviks," "Returned Students," or "International Faction." Ch'en Shao-yü assumed leadership of this group from the outset, successfully maneuvering to promote Pavel Mif to succeed the purged Radek. The deposed CCP leader Ch'ü Ch'iu-pai was involved in the opposing faction, with the ultimate upshot that when the two groups returned to China, Ch'ü joined the discredited Li Li-san against the "28 Bolsheviks," rather than dumping Li as Moscow had intended. Factional coalition-building was thus a highly complex affair.

Contrary to common belief, Sheng informs the reader that the "28 Bolsheviks" did not rise immediately to power upon their return to China, nor did they remain an entirely cohesive power bloc thereafter. Most returned to Shanghai sometime in 1930, and they did not seize power until the Fourth Plenum of the Sixth Central Committee in January 1931, with the help of their old rector Pavel Mif, now in China as Comintern representative. Rumors to the contrary notwithstanding, Sheng states that Ch'en did not betray the hapless Ho Meng-hsiung faction, which was executed by KMT secret police shortly after splitting from the party over the dispute that occurred at this conference. During the ruthlessly effective White terror of the early 1930s, Ch'en Shao-yü lost his nerve and sequestered himself in the top floor of a private sanitarium near Shanghai, fleeing to Moscow after scarcely a year at the party helm. In the subsequent struggle with Mao Tse-tung over party leadership, the faction split, with Chang Wen-t'ien joining Mao at Tsunyi in return for a promotion to the post of general secretary. Chang again sided with Mao in 1937 when Ch'en Shao-yü returned to China with the evident intention of resuming leadership of the movement (and replacing Chang as general secretary). The subsequent showdown between Ch'en and Mao seems, according to Sheng, to have concerned political rather than ideological issues, for he denies that there were disagreements over the nature of the Anti-Japanese National United Front. The author must base this judgment on published documents alone, inasmuch as he himself left the party in 1935.

Political intrigue and factional maneuvering

provide the pyrotechnics in this study-memoir, but it contains much else besides. There are informative chapters on the university's internal organization, budget, curriculum and pedagogical techniques, living arrangements, and party rules. Sheng writes evocatively and discursively, although the book is redundant, repetitive, and poorly organized. The text unfortunately suffers also from inadequate proofreading. The author makes little attempt to analyze, generalize, or draw conclusions from his experiences, apparently leaving this task to the reader. Sheng himself seems to drop from the narrative around 1932, providing no account of the circumstances that occasioned his leaving the party three years later. But all things considered, he is to be congratulated for this interesting little book, which adds to scholars' knowledge of a period about which very little information has appeared.

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ROBERT BOARDMAN. *Britain and the People's Republic of China, 1949-74*. New York: Barnes and Noble. 1976. Pp. xi, 210. \$23.50.

This work is one of a number of high-quality recent monographs dealing with British Asian policy during the past half-century. It offers a useful synthesis, written without any great passion but with a large measure of good sense. The author provides a systematic review of the major events with special emphasis, quite rightly, on the earlier period from 1949 to 1955. He remains calm, as objective as possible, and sensitive toward the factors that helped make and condition the conduct of British relations with China. Though Robert Boardman carefully uses a wide variety of published sources, he does not go beyond them; I cannot help wondering what revelations still lie buried in various archives.

The author discusses a number of issues that are timely, if only because they dramatically contrast the differences between the style, content, and particularities of British and American diplomacy. Such problems include the question of recognition, attitudes toward the Geneva Conference, the trade embargo, the status of Taiwan, the Quemoy and Matsu crises of 1954-55 and 1958, China and the United Nations, and the role of China in the cold war. Boardman concludes that the British were altogether more restrained, professional, realistic, and intelligent in their dealings with the Chinese than were the Americans. His handling of the complementary, often antagonistic, interplay between Britain, the Commonwealth nations—especially India—and the United States is excellent.

Boardman may be right in arguing that the Chinese revolution produced greater problems for British politicians, businessmen, and diplomats than did the first great revolution of this century; normalization of relations between Britain and China required more than two decades of tortuous negotiations and policy shifts. It is not surprising, therefore, that the period easily makes for a fascinating, analytical chronicle. I wonder when scholars will have a similarly clear exposition of American policies toward China during the same period and of those internal and external forces which shaped them.

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STEPHEN UHALLEY, JR. *Mao Tse-Tung, A Critical Biography*. New York: New Points. 1975. Pp. xiv, 233. Cloth \$9.95, paper \$5.95.

RICHARD BAUM. *Prelude to Revolution: Mao, the Party, and the Peasant Question, 1962-66*. New York: Columbia University Press. 1975. Pp. 222. \$10.00.

Stephen Uhalley's "critical" biography seems to have fallen between two stools. On the one hand, it tries to suit popular taste and meet popular demands by simplifying many complex issues and yet fails to adopt an attractive and readable style. On the other hand, it uses the method of summarizing published secondary works to add practically nothing new to our knowledge or understanding of this great political leader.

The book purports to be critical, but a few examples will show how uncritical Uhalley is. He analyzes (p. 131) the decision made at the Sixth Plenum of the Central Committee of the party that Mao would not be a candidate for another term as the chairman of the People's Republic of China. Some scholars feel that Mao was compelled to make the decision in the wake of the "alleged fiasco" of the Great Leap Forward; others suggest that he wished to free himself from the burdens of that office so that he could concentrate more on theoretical problems and long-term strategies of China's social change. Between these two interpretations, Uhalley thinks "it is reasonable" to accept the latter by giving Mao's "Sixty Points on Working Methods" as his evidence, but he gives neither corroborating evidence nor an analysis of the circumstances which led to this crucial decision. On the confrontation between Mao and P'eng Teh-huai at the Eighth Plenum our author once again equivocates. He says no more than that "the Party backed Mao to condemn P'eng and his 'anti-Party clique'." In a similar fashion, his treatment of Liu Shao-ch'i in the Cultural Revolution, of Lin Piao in 1971, and of the Russian threat



to China leaves the readers with a deep sense of frustration. How could Liu, the accused "capitalist roader," have hidden in the Party for so long? Why was Lin interested in "the burden of the office of the chairman of the People's Republic," particularly its ceremonial responsibilities, which, according to Uhalley, Mao had been anxious to avoid? Was Lin a conspirator as he is accused? For what reason did the USSR want to threaten the People's Republic?

Whenever Uhalley lacks the solid evidence required by a historian, he falls back on journalistic clichés like "apparently," "reportedly," or "according to Mao" in order to avoid the difficult task of judgment. Occasionally he even fails to see the distinction between biography and autobiography as this passage shows: "For a man who had already come to a realization of the road to successful revolution in China, Mao, one can imagine, could only have shaken his head at this point" (p. 33). The shaken head, to be sure, is Uhalley's.

Richard Baum's *Prelude to Revolution* is an outgrowth of his doctoral dissertation and a continuation of his monograph, *Ssu-ch'ing: Socialist Education Movement of 1962-1966*, written jointly with F. C. Teiwes. Unlike Uhalley's biography, Baum's study is obviously meant for the specialists. It is a detailed analysis of the political currents and undercurrents in the mass-education movement of that period. The contrast between Mao's method of controlling excesses of spontaneous capitalism and cadre malfeasance in the villages (a method of control from below through all-out mobilization of the peasant masses) and Liu's (a method of control from above through reliance on the formal party apparatus) is clearly laid out in the book (especially pp. 81 and 111). Baum's description of the Taoyuan Project led by Wang Kuang-mei (with outside financial help to the tune of US\$250,000 spent on such "frivolous" projects as the construction of a paved road, two power pumping stations, three wells, and an irrigation canal in the [production] brigade) reminds one of the Tinghsien Experiment led by Dr. James Yen in the 1930s.

Baum's major contention is that in the four years before the unleashing of the Cultural Revolution, Liu Shao-ch'i and Teng Hsiao-p'ing deviated from the Maoist orthodoxy, but nonetheless did not commit any conspiracy to restore capitalism in China. Dull and weighted down with irritating split infinitives, Baum's book is nevertheless a useful addition to the already enormous body of literature on Chinese communism.

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YUAN-LI WU. *The Strategic Land Ridge: Peking's Relations with Thailand, Malaysia, Singapore, and Indonesia*. (Hoover Institution Publications, number 147.) Stanford: Hoover Institution Press, Stanford University. 1975. Pp. vii, 97.

This short study seeks to explain the policy of the People's Republic of China toward four Southeast Asian countries that bridge the strategic Strait of Malacca and that (with the Philippines) constitute the five member nations of ASEAN or the Association of Southeast Asian Nations. Writing before the 1975 Communist victories in Cambodia, South Vietnam, and Laos, Yuan-li Wu, an economist at the University of San Francisco, focuses his analysis on policy changes by Thailand, Malaysia, Singapore, and Indonesia in response to the American withdrawal from Vietnam and to the evolution of the Sino-American détente. Emphasizing the geopolitical significance of the area for maritime transportation and naval power (especially as regards vital natural resource imports for Japan) and even as a "land bridge" for possible Chinese expansion, Wu briefly describes the relationships between Peking and each of the four countries during the years immediately before and since the Chinese Great Proletarian Cultural Revolution of 1966-69.

It is clear that Wu is not at all happy about the withdrawal of US forces from Southeast Asia or enthusiastic about increased Chinese influence in that region. In spite of occasional disclaimers to the contrary, Wu implies that the PRC virtually controls the activities of local Communist revolutionaries in the four countries, and he suggests that there is every chance that Peking will use them for "expansionist" purposes. Equally controversial is his suggestion that almost any domestic communal conflict is PRC-inspired. Having pointed to the significance of the Chinese ethnic populations in each of the countries, Wu describes Peking as persistently exploiting racial differences and purposefully creating communal problems for the four non-communist governments.

On the other hand, the author accurately describes Chinese policy toward Thailand and Malaysia as having "two different levels," i.e., at the same time attempting to establish relations with Bangkok and Kuala Lumpur on the basis of "peaceful coexistence," while supporting "national liberation" revolutionary alternatives to those same governments. Wu relates how the several countries have designed their initiatives to the People's Republic principally with the objective of trying to convince Peking to give up its support for revolution as a condition for friendly government-to-government ties.

Contrary to the analyses of other scholars who



have seen the best hope for political stability in these four countries to be in joint efforts under ASEAN to achieve multilateral arrangements for the neutralization of the area, Wu calls for renewed commitments largely along cold war lines. The study concludes urging support for an independent Taiwan as a "counter-attraction" to the PRC, and prescribes a revitalized US economic and military build-up in Southeast Asia to counter both Chinese and Soviet influence in the area—as Wu in his final sentence warns, "before it is too late."

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ALFRED C. OPPLER. *Legal Reform in Occupied Japan: A Participant Looks Back*. Princeton: Princeton University Press. 1976. Pp. xx, 345. \$20.00.

Born in Alsace-Lorraine in 1893, Alfred C. Oppler pursued a highly successful legal career in Germany until 1939, when he emigrated to the United States. After an interval of five years in Cambridge, he became a civil servant in Washington, and then in January 1946 joined the Occupation in Japan. His role in legal and judicial reforms (other than the constitution) was important, and he stayed on in Japan until the end of 1959.

Appearing seventeen years after his departure, this book presents a coherent account of his work in Japan. Over the years, Oppler has published a number of articles on the more important aspects of his work (this volume lacks a listing of these writings), so there is little new material here. His view of the American role in the making of the Constitution has changed since he wrote earlier, and he has written a thoughtful concluding chapter on the performance of the Japanese courts since the Occupation.

The major contribution of this book is the personal note, the sense of Oppler the man which frequently (though perhaps not frequently enough) breaks through the rather dry prose. Despite serving in Japan for fourteen years, Oppler never learned Japanese; but he seems to have had an instinctive sympathy for the Japanese, a lack of ethnocentrism, and little racism. These characteristics combined with a fair-minded approach and a broad grasp of issues to keep Oppler's admitted "evangelical liberalism" somewhat in check.

The critics of the Occupation, including myself, have had their fun with the Occupation's simple-minded ethnocentrism. Oppler's memoir reminds us that we cannot dismiss the Occupation quite so lightly.

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JAMES B. PALAIS. *Politics and Policy in Traditional Korea*. (Harvard East Asian Series, number 82.) Cambridge, Mass.: Harvard University Press. 1975. Pp. 390. \$18.00.

It is interesting to reflect on the different manners in which parts of the old orders in Eastern Asia—China, Korea, and Japan—responded to internal culture change and external culture impact. All were products of the same great tradition which produced the Confucian family of nations. Yet each worked out a unique process of modernization.

It is James B. Palais' theory that the same factors which made possible the remarkable longevity of the Yi dynasty (1392–1910) in Korea also contributed to the weakness of the regime. The crucial period 1864–73 was one in which the monarchy attempted to increase and expand central power at the expense of a powerful aristocracy. The attempt failed and 1874 saw a rebirth of bureaucratic and aristocratic dominance. This meant that when the "hermit kingdom" was opened two years later, the country was poorly suited to the attainment of modern national objectives.

In a sense this excellently researched and well-documented treatise consists of political biographies of Yi Ha-ŭng, better known as the Taewongun (Grand Prince), and his second son, King Kojong. The Taewongun acted as *de facto* regent from 1864 to 1873, even though he had no institutionalized position either by custom or statute. Rather, he acted for his son with the support of the Confucian principle of filial piety until Kojong took over leadership from 1874 to 1876.

The study reaches well beyond the varieties of personal leadership, however. It focuses on fundamental features of the traditional polity: social and economic organization; the nature and basis of political power; the influence of ideas (particularly neo-Confucianism) on policies and action. Palais thus provides a useful foundation for further study of the relationship between tradition and modernity in Korea.

Based on a wealth of original sources, the book explores in depth the private academies (and their Confucian theories), land and taxes, grain loans, the mismanagement of money, the court, the bureaucracy, and the politics of the *yangban* elite. The volume ends with the beginning of a fateful relationship at Tongnae in November 1868, when the Japanese envoy arrived to herald the Meiji restoration.

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NARENDRA NATH BHATTACHARYYA. *Ancient Indian Rituals and Their Social Contents*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. xvi, 184. \$12.75.

Almost a century ago the late F. Max Muller pointed out that "the origin and growth of sacrifice is an important page in the history of the human mind, and nowhere can it be studied to greater advantage than in India." (*Lectures on the Origin and Growth of Religion*, 1878, p. 146). Over half a century ago, A. B. Keith discussed the two important aspects of the Vedic sacrificial ritual, namely as spells and magic, in some detail (*Religion and Philosophy of the Veda*, 1925, 1: Pp. 260 ff.). The present work deals with an old subject but in a new dimension, that of the exploration of the social contents of the sacrificial and other rituals embodied in the texts of Brahmanical and popular religions and cults. Of its eight chapters the first five treat the nature of some of the great sacrifices, the *As'vamedha*, the *Rājasūya* and the *Vājapeya* and rites of passage, the *Upānayana*, and those associated with puberty. The remaining three chapters discuss the folk cults (phallic—*Linga* and *Yoni*), sacred prostitution, mother goddess, the cults of the dead, the *Pīr* cult, *Holi* and *Diwāli* festivals, and Hook Swinging in Bengal. The range of the contents of the work spans a time period of two millennia, and its sociocultural spectrum is equally impressive, covering the hieratic elements at one end and the so-called "proletarians" at the other.

Simply stated, Narendra Nath Bhattacharyya's major argument is that many of these rituals conceal behind their priestly garb social changes and movements from preclass ("primitive communism") to class societies and that most writers on the subject have missed this meaning. The author's method, the reader may suspect, consists of setting up straw men only to knock them down with references to works on anthropology and sociology, most of which were published during the latter part of the nineteenth and the earlier part of the present century. A case in point is his discussion of the term *rājan*, generally translated as king, which the author rightly contends is not only inadequate but quite misleading. His remarks on the fertility associations of some of these rituals are also well made, and his documentation generally impressive.

The weakest part of the work is that dealing with Buddhism and Jainism; the author's cursory acquaintance with original texts is easily apparent.

My chief criticism is that the author does not seem to be aware of the great deal of work done in religious history, anthropology, and sociology since the 1930s, and the omission of the work of Max Weber and Grawati Karve is particularly glaring. Nor does he feel the need of a rigorous

definition of key terms such as class, tribe, and clan in a work said to be a "departure" from the general run of Indological studies. He has, however, gathered an impressive conglomeration of ritual, sacral, and sociological data that South Asianists will certainly find interesting.

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M. N. PEARSON. *Merchants and Rulers in Gujarat: The Response to the Portuguese in the Sixteenth Century*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xii, 178. \$12.00.

"Wars by sea are merchants' affairs and of no concern to the prestige of kings," observed Sultan Bahadur of Gujarat (1526–37). This lofty disdain for seapower epitomizes the whole ethos of the Indian Muslim rulers and their nobility. Control over land was the crucial resource; and for a Gujarati nobleman, social honor and prestige derived from the size of his cavalry contingent. For most of the sixteenth century the Portuguese were able to control and tax the maritime trade of the Gulf of Cambay by imposing their *cartaz* (passport, navicert) system in all merchantships in this region. With the capture of Bassein (1534), Diu (1535), and Damão (1559), the Portuguese coastal flotillas were able to patrol effectively along the Gujarati coastline. Their ruthless devastation of the coastal towns in 1529–34, and again in 1546–48, showed to what lengths they were prepared to go in enforcing the Lusitanian crown's grandiloquent claim (dating from 1499) to the overlordship of the "conquest, navigation, and commerce" of the Indian Ocean. No effective resistance was to be expected from the timidly peaceful Gujarati traders, whether they were Muslims or Hindu *vanias*, since they were notoriously unwilling to fight under any circumstances. But neither did the warlike rulers and nobility of Gujarat react strongly to their kingdom's maritime trade being controlled and taxed by infidel foreigners. Yet the powerful rulers of Gujarat certainly could have exerted effective pressure on the Portuguese to drop their *cartaz* system, just as the Mughal emperors could have done later in their turn. They refrained from doing so because they did not consider that the Portuguese control of seaborne trade threatened their own purely land-based interests.

The details which M. N. Pearson gives of the working of the Portuguese *cartaz* system form a valuable contribution to the maritime history of Asia; but for Indian history his analysis of the varied nature of the Gujarati response is even more interesting. He shows that the inhabitants of Gujarat enjoyed considerable autonomy within their social groups, whether they were merchants, arti-

sans, farmers, or *stifts* (Muslim mystics). Thus, merchants could oppress each other, trade with the Portuguese in time of war, set prices and holidays, and pay customs-duties to the Portuguese at Diu. None of these activities affected the rulers, and so the rulers took no notice of them. In this disaggregated society, attachment was felt to one's own social group rather than to the kingdom of Gujarat. This satisfying and innovative study is rounded off with a useful bibliographical essay. It can be confidently recommended to anyone interested in the history of premodern India.

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to reconsider the standard vision of nineteenth-century Madras. As the author has shown, far more was taking place in this presidency than has usually been admitted or understood. As with any pioneering work, many more questions have been raised than answered, but this is to the credit of the author. He has opened new areas for future research while contributing a sound historical study which lays the foundation of that research. Of particular necessity now is a study that will bridge the gap between this work and Eugene Irschik's examination of politics in twentieth-century Madras.

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R. SUNTHARALINGAM. *Politics and Nationalist Awakening in South India, 1852-1891*. (The Association for Asian Studies: Monographs and Papers, number 27.) Tucson: University of Arizona Press. 1974. Pp. xix, 396. Cloth \$7.95, paper \$3.95.

R. Suntharalingam set out to provide "an interpretive account of the origins of nationalism in South India during the second half of the nineteenth century" (p. xi), and he has achieved this goal admirably. This work is a careful, well-documented, and readable account of the development of political awareness and its concomitant organizations in south India. The author has a strong attachment to Indian nationalism, which he sees as the product of new elites within the society of the Madras presidency. He traces the rise of an administrative elite, politically aware but tied by its values and occupation to the British-Indian governmental structure. He contrasts this to a later professional elite, more radical and aggressive than its predecessors. The interplay of these two elites with the British-Indian government at both the provincial and central levels is carefully examined. Suntharalingam also documents the forces within Madras City and throughout South India that shape political awareness and action.

His writing is lucid and his judgment sound. His research is firmly based on an extensive use of government documents, organizational records, private papers, newspapers, journals, books, and polemical tracts. Vernacular sources in the languages of South India are missing and would have added considerably to this study, but these materials can always be examined in future research. What he has accomplished here is significant. This book fills a void in our knowledge, since accounts of political history in this place and during this time are almost nonexistent. Any historian interested in modern South Asian history must give this book careful attention, and he may well be forced

C. COLLIN DAVIES. *The Problem of the North-West Frontier, 1890-1908: With a Survey of Policy since 1849*. New York: Barnes and Noble Books. 1975. Pp. xviii, 220. \$12.50.

This book is for today's reader both a useful historical study and an imperial period piece. Davies wrote the book in 1932 as a policy study of British actions on the frontier and displayed major concern for British power and its permanence. As a historical study it is useful because Davies, a historian, had military experience on the frontier, had access to confidential government reports, and was interested in historical geography. The present edition is a reprint of the original edition, with the addition of a succinct summary of frontier developments from 1908 to 1947 and a short additional bibliography. The book deals with four interwoven themes: Russian expansion toward India; British imperial policy on the frontier; the nature and restlessness of the border tribes; and Afghan developments. The Russian threat is dealt with briefly. Major attention is given to British frontier policies and to the tribal situation, with the Afghans brought in as a tertiary factor. The situation on the frontier from 1849 to 1907 was a confusing one, but Davies did as well as could be expected in presenting a clear and coherent story. In analyzing the sources of tribal unrest, he pointed to a mix consisting of an inhospitable and unproductive environment, fanatical anti-British religious leaders, the political ambitions of tribal and Afghan leaders, and certain British actions. In regard to British policy he believed that the frontier was best seen as a zone over which the British should exert control, while adapting policies toward the tribes to the needs of various regions. He was most critical of the British "forward policy" of the 1890s, which led to serious revolts, while praising the restrained policies of Lord Curzon after 1900. Although it would be well to supplement Davies' book with a more recent study of the frontier area,

it remains a worthwhile historical monograph as well as a representation of an imperial viewpoint that itself is part of history.

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RICHARD I. CASHMAN. *The Myth of the Lokamanya: Tilak and Mass Politics in Maharashtra*. Berkeley and Los Angeles: University of California Press. 1975. Pp. viii, 246. \$12.50.

Historians of Indian freedom movements have recently begun analyzing nationalist political organization in considerable depth. Richard I. Cashman's study of Lokamanya Tilak, the most prominent of the Maharashtrian nationalists, is a valuable contribution of that type, perceiving Tilak's innovations and limitations in terms of his relations to his highly varied constituencies. Cashman is at his best in presenting detailed data on the many caste, religious, and occupational groups that had to be mobilized and united before the nationalist movement could pose a serious threat to British hegemony. Scholars have generally agreed that Tilak was the first in his region to understand that power required active mobilization of large numbers of people at rallies, festivals, and the polls. Most of Tilak's biographers, until Cashman, have portrayed him either as the charismatic liberator of his people or as a nearly hysterical demagogue. In contrast, Cashman's rhetoric is mercifully free of hyperbole of either sort. He achieves this in part through careful analysis of the secondary leadership of Tilak's radical movement as it developed and declined between the 1890s and the 1920s.

One of Cashman's most persuasive conclusions is that Tilak managed to coalesce a wide variety of lieutenants, both urban and rural, Hindu and non-Hindu. The weakness of this arrangement became apparent after his death in 1920, for none of the others could hold the coalition together, especially since Tilak himself made no effort to groom a successor.

Somewhat parallel to this pattern is Cashman's convincing verdict on Tilak's relationship to constituencies that were not natural to his position as a Brahmin of Poona: the non-Brahmin masses, the industrial labor force of Bombay, and the non-Hindu elements of Maharashtrian society. Tilak pursued his primary goal, independence from Britain, so singlemindedly that he equivocated on all other issues of political importance. His brief effort to mobilize Bombay workers came to virtually nothing, since he was ill-informed about, and probably uninterested in, the economic issues that

divided management and labor. In this sphere, as in Hindu-Muslim relations, Tilak usually found it expedient to blame the British for intergroup conflicts within India. In this he shared the inadequacy of most Indian nationalist leaders, who failed to confront the social and economic conflicts that have become the dominant issues of Indian public life since independence.

Finally, Cashman succeeds in identifying the various elements of the extremist political culture with specific allies of Tilak. Here again he moves beyond earlier rhetoric about extremism to specific analyses of Maratha aggressiveness, the political implications of the physical training in gymnasia, and the elitist pretensions of some of Tilak's associates. He is thus able to illumine Maharashtra's ambivalent reception of Gandhi's non-violent politics after 1920—an era that now invites the careful scrutiny which he and others have given to the previous half-century.

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JOHN F. CADY. *The History of Post-War Southeast Asia*. Athens: Ohio University Press. 1975. Pp. xxii, 720. Cloth \$15.00, paper \$8.50.

In 1964, John F. Cady, the senior American historian of Southeast Asia, published the first historical survey of that troubled area by an American scholar: *Southeast Asia: Its Historical Development*. In that volume he covered the scene from earliest times mainly through the Japanese occupation period of World War II. Cady's new book carries his narrative from the last days of the war to the early 1970s, approximately a twenty-five year span. He addressed both volumes to "college students" and "the general reader." Since I have taught about the political history and economic development of Southeast Asia to graduate students for the past quarter-century, I can attest to the usefulness of Cady's two surveys, but I have yet to encounter that mythical (?) general reader who would survive the detail of these or other similar surveys by Americans or Europeans.

Cady is keenly aware of the fact that the serious student cannot get by solely on the reading of his or other surveys. This is especially the case for *The History of Post-War Southeast Asia*. The process of political, decolonialization and the correlative task of establishing viable political, economic, and social institutions, the sources and conduct of internal and externally stimulated and supported insurgency confronting fragile, newly independent polities throughout the whole period, the many occasions of coups and counter-coups, of large-scale but non-nuclear international war, the impacts of big-power rivalries and questionable

changes of big-power policies in the area—all this and much more have produced mounds of data, inferences, speculations, and value judgments. Anyone, including Cady in this volume, could hardly avoid error or at least substantial debate.

To take a single though complex example, on the last page of his text, Cady writes that “intelligent and concerned newsmen . . . have provided virtually all of the sources available for the concluding chapters of this study.” I find this an honest but shocking admission since, with few notable exceptions (for example, Tillman Durdin, Robert Shaplen, Dennis Warner), I found during my many years of returning to the field for on-site observation and study that the newsmen assigned to Southeast Asia by the American media (printed and electronic) were most frequently novices who went there for some short-term experience and then got out fast!

This issue is related to Cady’s present survey, more particularly to his handling of the Vietnam War—obviously the most controversial issue during the period. He sees Dulles as “the Puritan” whose struggle against “godless communism” produced a kind of “Manicheanism” for U.S. policy. Dulles’ views are linked with and presumably supported by American Catholics, especially Cardinal Spellman and Jesuit institutions (p. 323). This is not history worthy of any historian. It is at best prejudiced talk of an angry man whose sympathies are obviously elsewhere.

Cady’s sympathies lead him to errors on the same subject. For example, he regards France as the “residual sovereign” (p. 306) over “the State of Viet Nam,” later called the Republic of Vietnam after Geneva 1954. The fact of the matter is that France had already signed treaties of independence with Laos and Cambodia before Geneva 1954 and had initiated such a treaty during the period of that ill-fated conference. No country at Geneva signed the “Final Declaration;” France did sign a “cease-fire agreement” with the DRVN (Hanoi), but that had nothing legally or otherwise to do with Article 7 (on elections) of the unsigned “Final Declaration.”

Cady is obviously unhappy—as are many of us but for various reasons—with U.S. post-World War II policy in Southeast Asia, but he slights or virtually ignores, except for a few very minor references, any treatment of SEATO in this big book and consequently confuses his reader by referring inaccurately to the Thanat Khoman-Dean Rusk Agreement of 1962 (p. 395) as a “bilateral defense treaty” (p. 338). There is no such *treaty* between the U.S. and Thailand. There is—or was—such an agreement signed by Thanat and Rusk. Cady knows as well as I do that a ratified treaty (such as SEATO, which in 1954 included Thailand among

the signatories) has the force of law, but an agreement is merely an executive arrangement not ratified as provided by our Constitution.

Variable views on highly debatable issues are inevitable when writing a history of such recent events. I have no reason to reject advocacy; in other political contexts I am also an avowed advocate. But in a history, especially by the dean of American Southeast Asian historians, I expect more balance and less advocacy unless the latter is clearly labeled as such.

Despite my decades-long disagreements with Cady on substantive issues of Burma, Indochina, and U.S. policy in the area, I unhesitatingly recommend that my students—and other Southeast Asian students—read his books. His contributions, including his newest book, will long rightfully appear in our bibliographies, at least until younger scholars come along and do better. And that is the way it should be.

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ALEXANDER B. WOODSIDE. *Community and Revolution in Modern Vietnam*. Boston: Houghton Mifflin Company. 1976. Pp. xi, 351. \$9.95.

With the end of the Vietnam War in the spring of 1975, the postmortem time is about to begin. The reasons for American failure and Communist success will now concern the scholar, not the policy-maker. The book under review is one of the first to appear with such a post-war perspective, and it is also likely to be one of the best. In *Community and Revolution in Vietnam*, Alexander B. Woodside has explored the internal dynamics of modern Vietnamese history in an attempt to place the war in historical perspective. A key reason for Communist success, he suggests, is the persistent urge of the Vietnamese people for “organized communities.” The need to find a means of unifying the people has been a common theme within the Vietnamese nationalist movement since early in this century. Non-Communist nationalists, however, were plagued by their urban orientation and endemic political squabbling, and found the solution elusive. It was the Communists who located the key: through institutions such as the collective farm and the functional mass organizations they have synthesized traditional attitudes with Marxist-Leninist organizational techniques, and victory in the South was their reward.

Woodside’s thesis is familiar, reminiscent in some respects of earlier works by Paul Mus and Frances Fitzgerald, among others. Where they were impressionistic, however, Woodside has based his conclusions on a solid understanding of



the Vietnamese past. While he has by no means answered all of our questions about the meaning of the war, he has provided a persuasive and well-documented interpretation of the conflict in its historical setting, and for that we should be grateful. The war may be over and Washington's policy in ruins, but it is not too late for Americans to learn the lessons of defeat.

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GEORGE H. KERR. *Formosa: Licensed Revolution and the Home Rule Movement, 1895-1945*. Honolulu: University Press of Hawaii. 1974. Pp. xviii, 265. \$12.50.

This is a general history of Formosa (Taiwan) under Japanese control from 1895 to 1945. After recapitulating the island's tormented past, George Kerr, a veteran observer of Formosan affairs, gives a chronological account of Japanese rule. The subtitle, *Licensed Revolution and the Home Rule Movement*, suggests the themes originally intended. The bulk of the book, however, concerns Japanese political development on the island and the elaborate system of control by which various governors-general and civil administrators regimented their second-class subjects. The author intermingles the description of political events abroad and in mainland China without regard for Japanese capitalist encroachment, post-1924 peasant-labor movements, and other important social and economic issues on the island itself.

The chapter on the licensed revolution gives a misleading impression of colonialism. Is it not a worn-out apologia that a colonial government brings its hapless subjects "revolutionary" change? Administrative reorganization, a standardized monetary system, public health programs, land reassessment, agricultural renovation, and construction of Western-style office buildings, railroads, factories, and schools—these were first designed with the intention of facilitating Japanese colonial control over the island, often at the cost of its inhabitants. This is exemplified by the Japanese policy of forcing Formosan peasants to cultivate more sugar cane, while allowing most of the profits from the increase to fall into the hands of *zaibatsu* capitalists.

The home-rule movement involved the establishment of various organizations (culminating in the founding of the Taiwanese People's party in 1927 and the Taiwanese Communist party the following year) and the sending of fifteen annual petitions to the National Diet at Tokyo from 1921 to 1934. Because the home-rule movement began after the failure of some eleven revolts during the first fifteen years of this century, a systematic treatment of these uprisings would undoubtedly

illuminate their general nature. In 1912, for example, people revolted in Lin-ch'i-p'u to protest exploitation by Mitsubishi; the uprisings of Lo Fuh-shing in the same year, as well as of Yü Ch'ing-fang and Lo Ch'un in 1915, were led by literate people who longed for the island's reunification with the mainland. In explaining the less-violent tendencies of Formosan resistance to colonial rule, Kerr suggests that economic and social gains "moderated Formosan reaction to alien controls" (p. 108), when it was for the most part Japanese high-handed suppression that stifled popular uprisings.

One might also question the historical validity of some of the author's statements: for example, that the Dutch left Formosa a "productive colony" (p. 5), that in Ch'ing times Formosans developed a "separatist tradition" (pp. 7-8), and that "bold and forthright foreigners [namely missionaries] tended to support Formosan rights and interests" (p. 12). The virtues of the book are further outweighed by an array of factual errors (for example, the "Great Rebellion of 1714" [p. 15], which in fact the island never witnessed) as well as mistakes in romanization (for example, Sung Sung [p. 155] instead of Tseng Tsung). In spite of these flaws, for those interested in the island's history, twentieth-century Japanese politics, and comparative colonialism, the book provides a fairly readable outline of Taiwanese political development from 1895 to 1945.

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SPENSER ST. JOHN. *Life in the Forests of the Far East*. In two volumes. Introduction by TOM HARRISSON. (Oxford in Asia Historical Reprints.) New York: Oxford University Press. 1974. Pp. xix, 400; xviii, 420. \$37.25 the set.

The eighteenth- and nineteenth-century European explorers once represented the essence of adventure and romance; certainly this was true for at least one middle-class American youngster growing up in the 1950s, for whom the idealized stories of these men of daring first fired an interest in the Orient. The world has changed much since then; youth has matured, and historians have exploded many colonialist myths. No longer is it possible to view the imperialist imperative—past or present, European or American—in such benign terms. The early explorers, intrepid agents of imperial power, now seem far less romantic and often a good deal more malevolent.

Nonetheless, the books written by these representatives of an earlier era and different consciousness—men like Spenser St. John—frequently have great value as documentary sources

for students of Asian and African history, recording not only the increasing Western penetration but also the characteristics of indigenous society and activity. Many are also successful in conveying the sense of adventure and discovery attendant upon European exploration and early colonization. St. John's book on a hitherto isolated region of Southeast Asia was among the best of the genre on both counts. For this reason specialists in Southeast Asian history should congratulate Oxford University Press for reprinting this and other long unavailable works in handsome new editions.

St. John was one of the most interesting figures of British imperialism in the Malay world: a diplomat, government adviser, amateur ethnologist, acute social observer, and hardy and courageous traveler in uncharted districts. *Life in the Forests* was published in 1862; despite its misleading title it concerns St. John's thirteen years of residence and travel in northern and western Borneo, in the states that today comprise east Malaysia (Sarawak and Sabah) and the sultanate of Brunei. The 1840s and 1850s were the most critical period of European penetration in that region, and St. John worked closely with the first white rajah of Sarawak, James Brooke. Not surprisingly, Brooke and his growing realm merit considerable attention in these volumes. For the historian and anthropologist, *Life in the Forests* constitutes one of the two or three most valuable Western-language sources on mid-nineteenth-century northwestern Borneo.

St. John was a reasonably fair-minded observer by the standards of the day, and he maintained a healthy respect for the peoples and cultures of Borneo. Yet he could not completely escape the prejudices of imperialism, viewing Malays as lazy, Ibans as "improveable" with Christian missionary effort, Chinese settlers as treacherous if unchecked, and Brunei government as decadent. Fortunately, St. John's work is redeemed by the extensive description of society and politics among various ethnic groups: Ibans, Kayans, Land Dayaks, Chinese, and Brunei Malays. The pioneering accounts of the human geography of Sabah's northwest coast and the Limbang River basin are particularly valuable contributions. *Life in the Forests* also contains a lengthy assessment of Christian missionary activity and a controversial account of the so-called "Chinese Insurrection" in Sarawak.

Sixteen striking illustrations, reproduced reasonably well from the original edition, enhance the two volumes. Like the original, the reprinted edition contains no index, although the value of the work for contemporary scholars would have been greater had one been included. In his rambling introduction the late Tom Harrisson captures the spirit of the work rather well. He also rightly casti-

gates modern-day charlatans who hoodwink the gullible public—in books and on television—with supposed "eyewitness" accounts of cannibals, headhunters, and other wonders in contemporary Borneo. Armchair travelers, escapists, and, more importantly, scholars are better advised to stick with St. John and his contemporaries, whose explorations were usually far more genuine and whose discoveries were less sensationalist.

CRAIG A. LOCKARD

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IAN WILLIS. *Lae: Village and City*. Melbourne: Melbourne University Press. 1974. Pp. xvi, 173. \$17.80.

JOHN ROBERTSON. *J. H. Scullin: A Political Biography*. Nedlands, Australia: University of Western Australia Press. 1974. Pp. xii, 495. \$18.50.

PETER BISKUP, edited with an introduction by. *The New Guinea Memoirs of Jean Baptiste Octave Mouton*. (Pacific History Series, number 7.) Honolulu: University Press of Hawaii. 1974. Pp. xiv, 161. \$13.50.

DIANE LANGMORE. *Tamate—a King: James Chalmers in New Guinea, 1877-1901*. Melbourne: Melbourne University Press. 1974. Pp. xv, 169. \$17.80.

James H. Scullin was the only leader of the Australian Labor Party to become prime minister of the Commonwealth during the period between the two World Wars. He had been leader of the party for only a year when he led it to a sweeping electoral victory, but unfortunately for him and the government he led, they took office in October 1929. The next two or three years were not a time for men in office to enhance what reputations they may have had. Scullin's was destroyed by the Depression, and he and his party were hurled from office by the Australian electorate after twenty-six months in power.

There was much injustice in this. Australian economic difficulties during the Depression were well beyond the capacity of any Australian leadership to resolve, rising from the depressed prices for the nation's leading export commodities in the world markets, and from the heavy burden of debt that previous generations had accumulated, particularly in London. The Scullin ministry was torn asunder by personal rivalries and ambitions, but even more by the conflict between its humanitarian and social welfare concerns and the overwhelming demands of the financial community for budget balancing, tax reduction, credit restriction, and above all, for meeting Australia's external debt obligations. In the end, Scullin accepted and defended the conservative course, under the conviction that it was either that or default. But it was a decision which required courage to sustain under the assaults of J. T. Lang, the demagogic premier

of New South Wales, and the wavering faith of many of the party members in the Commonwealth parliament. Amid all these storms Scullin moved with dignity, and at a time when the hot gossellers of varying nostrums were burdening the air, he continued to speak in moderate tones and always to unite rather than further divide a distracted people. Although overwhelming forces were arrayed against him, Scullin emerged with his integrity intact, and not without some measure of success in abating the forces of economic disaster.

John Robertson faced problems in the writing of the biography, mostly arising from the fact that Scullin retained none of his personal papers. He writes of the political man, but since there was such a basic integrity to Scullin, and since his adult career was almost completely in the political field, the work is not one-sided. The author's judgments of the Australian political scene, and his measurement of the strengths and weakness of his subject are sound and judicious. It is a book worthy of that subject, and an excellent addition to Australian biography.

Though Scullin held the portfolio of External Affairs in his ministry, and in more ordinary times might have been concerned with the Australian-ruled territory of Papua and New Guinea, none of that appears in this biography. But the recently attained independence of the former Australian territory does make the history of that land and its people of greater interest. Diane Langmore has done a service in reviewing the life of James Chalmers, the London Missionary Society's leader in the establishment of missionary enterprise in the southeastern area of New Guinea. There was nothing pallid about Chalmers; the meekness enjoined on Christians was rarely his. He would have been, as some of his acquaintances remarked, a great admiral or even buccaneer. And he did not spare his superiors in the LMS the advantage of his criticism, a fact glossed over by some earlier Chalmers biographers. Langmore provides both a scholarly and readable life of this lively exemplar of Christian missionary effort in the Southwest Pacific.

Traders as well as missionaries were the instruments of European hegemony in the Southwest Pacific, and the rather recently discovered memoirs of Jean Baptiste Octave Mouton give a first-hand account of many aspects of European enterprise in the New Britain region from the viewpoint of an active and successful participant in the trade. Peter Biskup's introduction and footnotes vastly enrich the memoirs.

Ian Willis's account of the development of Lae is a good piece of local historical writing. The emphasis is on the early years of the settlement, with only passing mention of recent growth. GIs who

passed through New Guinea would doubtless be surprised to learn that Lae is today the second largest town in New Guinea and that it has a university, and I wish that the author had paid more attention to this recent growth.

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## UNITED STATES

WILLIE LEE ROSE, edited with commentary by. *A Documentary History of Slavery in North America*. New York: Oxford University Press. 1976. Pp. xvi, 537. \$19.95.

It will be some time before another documentary exploration of the many dimensions, black and white, of slavery, as freshly conceived and thoughtfully executed as this one, appears in print. From the opening document on the arrival of the first blacks into English colonial America to the folk tale that closes the book, there is absorbing and oftentimes painful reading. Concerned with interior and external aspects of slavery, with psychic and spiritual damage to slave and master, and with slavery's mode of operation and institutional significance, Willie Lee Rose offers a volume that, through its juxtaposition of documents, enables the expert and the general reader to consider main problems in the field.

Documents included cover, among other topics, slavery in the Colonial period, the Revolutionary period and beyond, servile insurrection and other forms of disaffection, the domestic slave trade, slave laws, work, and management. One finds in the section on the Revolutionary period, for example, all the eloquence of Thomas Jefferson at the service of racism. Such things and more make the Rose volume fascinating reading. With a deft touch and sensitive appreciation she has put together in arresting sequence, in "After hours . . . Beliefs and Amusements," documentation on slave music, dance, religion, and tales, which shows that even in the face of brutal enslavement the human spirit will soar to assert its presence.

Rather than the by now conventional documentary emphasis on assessments of slavery, Rose turns in this volume to the materials scholars mine in the field. And while her discoveries are not as revolutionary as those in a handful of earlier great documentaries on black oppression—particularly the works of Carter G. Woodson and Herbert Aptheker—she nonetheless presents in print numerous documents for the first time. Rose challenges those planning work along roughly comparable lines, for seldom does one find such flashes of

erudition prefacing each document, providing the whole with the glow of authority.

Some specialists, however, will not be persuaded by the assertion, advanced in "Revolts, Plots, and Rumours of Plots," that Gabriel's conspiracy was "the most sophisticated in political intention of all the slave plots uncovered" (p. 107). Still other scholars of slavery (but not many, considering the virtual unanimity on the subject) will doubt that it was primarily Christianity which "caused slaves to think" deeply about the injustices of their oppression (p. 462). In fairness to Rose, this last problem must be seriously studied, for seldom is the West African's sense of his humanity considered in explorations of the sources of Afro-American discontent in slavery.

In sum, a scholarly standard for documenting slavery has been set.

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BROOKE HINDLE, editor. *America's Wooden Age: Aspects of Its Early Technology*. Tarrytown, N.Y.: Sleepy Hollow Restorations. 1975. Pp. vii, 215. \$15.00.

When the frontier contained forests, it made possible a technology based on wood and "a society pervasively conditioned by wood" (p. 3). That type of society developed during the first two hundred years of American history, from the time of the first settlement to the coming of steam and steel in the nineteenth century.

The essays in this book represent the diverse research interests of six authors. The result is a book shaped by the contributors' interests, not by a central theme. The essays cover the forest society of seventeenth-century New England, early lumbering (a pictorial essay), woodworking, wood craftsmanship, the construction and operation of colonial watermills, and waterpower in the nineteenth century. The two most interesting and useful articles are the first and the last.

Charles F. Carroll's essay is about the seventeenth-century forest environment and the colonists' terrifying experience with wolves, cougars, and blood-sucking flies. The experience was awful enough to send some settlers back to the safety of England. But for those who stayed, the riches of the forests drew them deeper into the wilderness, and they initiated what Carroll terms timber imperialism—the expansion of settlement in order to mine the woods. His thesis is that the economics of timber and the environmental effect of the forest acted as a force in forging the American character.

In the concluding essay, Louis C. Hunter argues that the prime stimulus of early industrialization in the United States was the wooden waterwheel

and not the steam engine. Power is essential to industrialization, but in the era of wood and water the American economy moved at a slower pace for which the waterwheel, subject to seasonal changes, was adequately suited.

The availability of an abundant supply of wood determined early American technology. Wood as a building material was not as significant as its use in producing power. For more than anything else, the source and application of power gives an age its distinctive character.

As Hunter's and Carroll's essays demonstrate, technology is of the same historical value as social, economic, and political factors. Just as social structure is an indication of the character of an age, so technology shapes an age.

HAROLD ISSADORE SHARLIN  
*Iowa State University*

JOSEPH A. GOLDBERG. *Shipbuilding in Colonial America*. Charlottesville: University Press of Virginia for the Mariners Museum, Newport News, Va. 1976. Pp. xiii, 306. \$15.00.

The importance of shipbuilding in the economic life of colonial America has been generally recognized; it is somewhat surprising, therefore, that Joseph A. Goldenberg's book is the first attempt to survey the topic in a comprehensive and systematic way. Previous reluctance to deal with the topic may, as Goldenberg asserts, be "attributed to the sketchy records now available" (p. x). Thus, the author is unable to deal definitively with every aspect of his topic; he has quite sensibly confined himself to "what can actually be documented" (p. xii). He has, nevertheless, brought together information from a surprising number and variety of sources; and it seems unlikely that this study will be superseded in the near future.

In his discussion of shipbuilding in the earliest colonies the author concludes that the industry arose largely out of local necessities. Expansion of shipbuilding in the eighteenth century, however, arose in part from the extraordinary English demand for shipping during war years but also because of the fundamental advantage of the colonial industry—its lower costs. Indeed, the building price per ton for a colonial vessel was often £2 to £4 less than a British-built one. In this connection, the author accepts the conventional estimate that, by the time of the Revolution, "at least a third of the British merchant fleet was American-built" (p. 125).

Many other topics are considered in the book, among them the various technical aspects of shipbuilding, the acquisition of capital, the nature, composition, and training of the work force, and the geographical distribution of the industry.



More than half of the volume consists of tables drawn from shipping registers and port books. Well-chosen illustrations enhance the value of a book which should certainly be on the shelves of all college libraries, not to mention those of maritime enthusiasts.

ARTHUR L. JENSEN  
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ALBERT E. VAN DUSEN. *Puritans against the Wilderness: Connecticut History to 1763*. (Series in Connecticut History, volume 1. Publication of the Center for Connecticut Studies.) Chester, Conn.: Pequot Press. 1975. Pp. ix, 150. \$3.50.

DAVID M. ROTH and FREEMAN MEYER. *From Revolution to Constitution: Connecticut, 1763 to 1818*. (Series in Connecticut History, volume 2. Publication of the Center for Connecticut Studies.) Chester, Conn.: Pequot Press. 1975. Pp. ix, 111. \$3.50.

JANICE LAW TRECKER. *Preachers, Rebels, and Traders: Connecticut, 1818-1865*. (Series in Connecticut History, volume 3. Publication of the Center for Connecticut Studies.) Chester, Conn.: Pequot Press. 1975. Pp. x, 95. \$3.50.

RUTH O. M. ANDERSEN. *From Yankee to American: Connecticut, 1865-1914*. (Series in Connecticut History, volume 4. Publication of the Center for Connecticut Studies.) Chester, Conn.: Pequot Press. 1975. Pp. x, 96. \$3.50.

HERBERT F. JANICK, JR. *A Diverse People: Connecticut, 1914 to the Present*. (Series in Connecticut History, volume 5. Publication of the Center for Connecticut Studies.) Chester, Conn.: Pequot Press. 1975. Pp. ix, 124. \$3.50.

As the editors of this series acknowledge, these five volumes are not intended to offer a "comprehensive or encyclopedic record" of Connecticut's past. Instead, the various authors offer a general narrative description of the principal forces and events within specific periods in the state's history. Each volume also provides bibliographical references, a time line, and a supplementary selection of some relevant primary sources. All the monographs contain pertinent illustrations, and all but the first volume contain some footnotes.

Albert E. Van Dusen surveys the origin and growth of Connecticut as a colony, from its English and Massachusetts Bay antecedents to the end of the French and Indian War. Van Dusen discusses the colony's main geographical features, its original Indian inhabitants, and the principal political, economic, religious, and social developments that occurred under its white settlers. He especially cites the effects of the Great Awakening and imperial wars upon the colony, and he emphasizes the considerable achievements of the colony's

able and energetic governor, John Winthrop, Jr. He provides a succinct account of Connecticut's complicated boundary disputes that took more than two centuries to settle. And, after noting the colony's small but expanding maritime activities, its growing religious diversity, and the extensive turnover within its legislative lower house, Van Dusen agrees with historians such as Richard Bushman and Oscar Zeichner that by 1763 Connecticut was no longer a "land of steady habits."

David M. Roth's and Freeman Meyer's volume examines the period from 1763 to the adoption of a new state constitution in 1818. Much of its subject matter—the colony's political-religious sectional controversies, the nationalistic urges of the Revolutionary era, changing educational and economic patterns, and an increasing outward migration—demonstrates a continuing unsteadiness within the Connecticut scene. Lifestyles for the period are discussed, and the independent, self-reliant spirit of the residents is portrayed in the career of Governor John Trumbull. A concluding chapter traces the downfall of Connecticut's Federalists and the formulation of the reformist Constitution of 1818.

Janice Law Trecker's study of the years 1818-65 centers on Connecticut's early transformation from an agricultural, inwardly oriented society to an industrialized, urbanized, and diverse population. Such changes were reflected politically in the rise and fall of several political parties seeking to capitalize on newly emergent issues. Essentially, however, prevailing attitudes remained conservative and allegedly restricted the extent of reform movements and benefits for the average citizen. When the Civil War came to Connecticut, the effects of the transformations became apparent as the state contributed manpower and volunteer support, and especially industrial productivity, to the Union triumph.

Ruth O. M. Andersen's survey of the era between 1865 and World War I details the acceleration of the pre-Civil War economic and social trends. Aided by power, rail, and shipping networks and the state's mechanical expertise, industry rapidly expanded and diversified, while agriculture became more specialized. Urbanization continued, and the predominant Yankee character of the state was altered because the exodus of its native-born citizens increased simultaneously with the growing number of new European immigrants. The problems created by the influx of these new immigrants and the issues of labor unrest and progressivism remained, unfortunately, largely unresolved during this half century. Connecticut's government, still controlled by big business and rural interests, failed to offer adequate leadership. Yet the author nonetheless concludes that "a quest



for regularity and predictability" by both old residents and new immigrants "gave unity in a time of turmoil."

The concluding monograph examines Connecticut from World War I to the start of Governor Ella Grasso's administration in 1975. As Herbert F. Janick, Jr. demonstrates, the state continued to be influenced by new forces in American life. The wartime enthusiasm of 1917-18 was followed by postwar economic dislocations, the Red Scare, and the development of an interlocking business and governmental relationship under the aegis of Republican boss J. Henry Roraback. The depression years were represented by the pragmatic leadership of Governor Wilbur Cross, the increased power of labor, and the converse decline of business and Republican power. World War II brought a period of stability, unity, and purpose, but the post-1950 decades produced renewed social changes and internal disturbances. Janick finds that these social transformations are reflected in the encroachment of suburban New York lifestyles and distinctive educational innovations.

This well-written series essentially succeeds in its modest aims. There are, however, some chronological mistakes. For example, the Republican party was founded in 1854, not 1852, and the Fifteenth Amendment was ratified after, not before, Connecticut public school segregation was outlawed in 1868 (vol. 3, pp. 44, 57). A reference to the Connecticut Assembly's modification of Congregational control in 1791 fails to note that such modification actually began in 1727 when Anglicans living within the parish of an Episcopal priest were excused from paying regular church taxes (vol. 2, p. 30). The second volume also tends to oversimplify the causes for the growth of Revolutionary sentiment in Connecticut, and it incorrectly asserts that by 1780 England was at war with the League of Armed Neutrality (p. 22). Documentation in the series is informative, but rather limited in scope. Yet, in the overall analysis, both editors and authors deserve commendation for providing such an edifying general history of the "land of steady habits." Perhaps similar worthwhile projects will be adopted in other states.

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Chicago*

SYDNEY V. JAMES. *Colonial Rhode Island: A History*. (A History of the American Colonies in Thirteen Volumes.) New York: Charles Scribner's Sons, 1975. Pp. xviii, 423. \$15.00.

Historians have not served Rhode Island well. With the exception of a few excellent monographs and a general history covering the first fifty years,

colony and state alike have suffered from benign neglect, special pleading, or incompetence. What stands out most impressively in this general history are the author's common-sense treatment of topics long and often profitlessly debated by professionals and amateurs, his wise skepticism about many of the so-called facts of Rhode Island history, and his coherent overview of Rhode Island's evolution from colony to statehood. Sydney V. James achieves all this with grace, wry humor, and many gems of shrewd insight. He knows his people, and he knows them for all of their human rather than legendary and heroic qualities. Especially praiseworthy is his treatment of the original settlements, religious controversy, land lust, Indian relations, and the evolution of both government and party politics. The analysis is throughout anchored in colonial rather than contemporary values. And it is this that makes the discussion of Roger Williams, liberty of conscience, and the separation of church and state, for example, so refreshing. What we see so clearly is that freedom emerged as a pragmatic solution to the turmoil of "otherwise-mindedness." This was no grand abstraction established out of libertarian principle, but a hard-headed accommodation with reality. The colony had to choose between survival and diversity. It was only later that the colonists made a virtue out of necessity.

James brings the same realism to his analysis of the colony's survival as a political entity and particularly to the complex, frustrating, and often delicate process of securing a royal charter, creating a functioning central government in the face of awesomely complex local factionalism, and defending the colony against the ambitions of its neighbors and the eagerness of the imperial government for order, conformity, and rationalization. Again, out of expediency came a workable government based on principles that were in many ways in advance of their times. It was only later, after the Revolution, that Rhode Islanders conferred on their charter and form of government an emotional reverence entirely out of keeping with the always delicate and sometimes grubby process by which it had come into being and had been nurtured and defended.

Rhode Island has at last found a general historian worthy of its subtleties, its place in the larger American scheme, and its prickly nonconformity. This is not to say that James has written the last word on the colonial period. A general history giving more emphasis to social and economic history would be especially welcome. The lives of ordinary folk, the processes of farm making, the rise of the maritime economy and its ancillary enterprises (distilling, iron-making, candle-mak-

ing, and the like), and the connections between country and seaport have still to be woven into the larger general story.

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JOSEPH L. BLAU. *Judaism in America: From Curiosity to Third Faith*. (Chicago History of American Religion.) Chicago: University of Chicago Press. 1976. Pp. xiv, 156. \$8.95.

An essay on American Judaism dealing with a minority which is both a religious and ethnic group, whose members immigrated to the United States from different places, and with a host country that never mandated separate corporate affiliation has built-in methodological problems of definition and approach. Joseph L. Blau side-steps some of the difficulties: he defines a Jew, irrespective of practices, as one who willingly accepts that identification, and he intertwines ethnicity with religion, interpreting ethnic associations, like membership in fraternal and Zionist organizations, as feeders or even surrogates for religious experience. More important, he contends lucidly and cogently that there never was or could be one normative Judaism in the United States because Jews imbibed four basic influences from American religious culture that stimulated and legitimized diversity: Protestantism (in the sense of denominationalism), pluralism, moralism, and voluntarism (get used to that spelling, the editor warns). What emerged from a confluence of those forces with the assorted cultural baggage brought over by Jews is a multiple American Judaism, including not only the familiar categories of Orthodox, Conservative, and Reform, but also Jewish Humanists, Scientists, and the "unsynagogued," whose religion may be philanthropy on behalf of non-Jews as well as for Jews. Blau, long a protagonist of pluralism, welcomes this diversity that he calls the source of cultural creativity.

It is unfortunate that the brevity of the volume leads to oversimplified explanations of social and cultural currents, particularly with respect to the evolution of Reform and Conservative Judaism and the course of American Zionism. There are also specific points that could be questioned: for example, Blau unsatisfactorily explains why Jews did not settle in colonial Boston, why Isaac Leeser advocated agricultural colonization in Palestine, and why Reform turned more ethnic in the 1930s. It would have been useful, too, if Blau had included up-to-date estimates of the membership of the different Jewish groups and assessments of their relative strengths.

Blau's forte lies in synthesis and interpretation, and those were his objectives for the book. Most of

the factual material can be found in the writings of Nathan Glazer, Marshall Sklare, Charles Liebman, and Blau himself.

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SAMUEL REZNECK. With a foreword by JACOB R. MARCUS. *Unrecognized Patriots: The Jews in the American Revolution*. Westport, Conn.: Greenwood Press. 1975. Pp. xiv, 299. \$13.95.

JACOB RADER MARCUS. *Early American Jewry: The Jews of New York, New England and Canada, 1649-1794*. Volume 1. Reprint. New York: Ktav Publishing House. 1975. Pp. xxviii, 594. \$35.00.

Prior to the pioneering work of Marcus Hansen, American historiography virtually ignored the presence of ethnic and religious minorities in American society. Studies dealing with the experience of such groups were generally produced by well-meaning amateurs whose work was invariably filiopietistic and often self-glorifying. Most of the material was published under the auspices of denominational societies with a two-fold purpose: to prove that the sponsoring group had arrived on the American scene at an early date and that it had always been patriotic.

The more threatened and excluded a minority group felt itself to be, the more likely it was to press the search for venerable American credentials. In 1892, when the tidal wave of East European immigration produced an undertow of anxiety among native American Jews, a group of Jewish communal leaders organized the American Jewish Historical Society. The early volumes of its publications are devoted almost exclusively to incidents in the lives of the small number of Jews who settled in North America prior to 1800. In 1895, Simon Wolf, a leader of B'nai B'rith, published a volume entitled *The American Jew as Patriot, Soldier and Citizen*. While much of this material can be characterized as what Jacob Marcus has called "ethnocentric schmoose," it represented the response of a group which felt itself ignored and neglected and sought to enhance its status in a troubled society by establishing its participation in a heroic national past.

With the passage of time and the broadening of the American cultural context, American historians became more conscious of the impact of immigrant groups, and students of Jewish history became more sophisticated in their understanding of social context. Serious students of American Jewish history might still deal with the numerically small Jewish presence in colonial America, but their objective was to understand the dynam-

ics of minority group behavior in a developing society and thereby to enhance our understanding of both the general society and the specific sub-culture.

The pre-eminent scholar of American Jewry in the colonial period is Jacob Rader Marcus. In 1951 he published a two-volume work entitled *Early American Jewry* which he regarded as an expression of the desire of historians "for a deeper and better understanding of the varied forces that helped mold American culture." The work marked a giant step forward in its comprehensiveness and the seriousness of its effort to deal with Jewry in colonial America in both its American and its Jewish context. The primary flaw of the work was lack of critical apparatus and adequate references which might assist students in building on Marcus' pioneering effort. Marcus was aware of the deficiency and indicated in his preface his "intention in a later more formal history of American Jewry, to present a completely annotated study." In 1970 a three-volume work entitled *The Colonial American Jew, 1492-1776* appeared. This study refined and enlarged Marcus' earlier work and provided the promised annotation.

Marcus' earlier history has been republished in one volume. There is some interest in comparing the two works and marking the development in Marcus' approach. The contrast is also instructive in indicating the change in the position of American Jewry and the growing seriousness and diminishing defensiveness of Jewish scholarship in America. There is historiographic interest in comparing a work of the early 1950s with one of the 1970s. Judged on their own merits, the more recent three-volume treatment is to be preferred on all counts to this reprint.

Samuel Reznick's *Unrecognized Patriots* is a latter-day example of the less attractive specimens of the older style of American history writing. It is anecdotal, repetitious and stylistically turgid, adding little by way of insight or information to previously published materials. Early in the volume, the author comments that "the Jews were not significantly involved in the events tharevolution [sic], since they were neither numerous nor important in Colonial America." One is tempted to put the book down at this point and conclude that the "unrecognized patriots" fully deserve their obscurity.

The work is marred by unsubstantiated reports concerning the individuals described. David S. Franks "may have attended" the academy in Philadelphia which "later became the University of Pennsylvania." He was "possibly with" Benedict Arnold at the Battle of Saratoga. "He [Franks] was perhaps with him [Arnold] in the Albany hospital where Arnold recovered from

serious wounds." "He was perhaps also for a time an aide to General Benjamin Lincoln in South Carolina." "He may have been briefly a courier for Washington." "He had apparently attained the rank of lieutenant-colonel." One of the few facts of Franks' life which is clear is that he ended his career as an assistant cashier in the Bank of North America and was buried in 1793 in a Christian cemetery, casting doubt on both his significance as a patriot, and his credentials as a Jew.

Perhaps the most interesting material in this volume on "unrecognized patriots" is that dealing with Jewish Tories who were loyal to the British crown. What is lacking is analytical material which might explain who among colonial Jews became Tories, why they chose this allegiance, and what impact the existence of this group had on the Jewish community and on the place of Jews in the newly established republic. Clearly a work designed to stress the importance of Jewish patriots cannot deal adequately with the problems of loyalist deviants, about whom a more interesting book might have been written.

Reznick's book concludes with the muddled observation that "since the Jews then were few and, except for a smaller number, not very important, their contributions, while not insignificant, were moderate if not altogether minor." One might apply the same judgment to this work. It is "altogether minor."

LEON A. JICK  
Brandeis University

GERALD F. DE JONG. *The Dutch in America, 1609-1974*. (The Immigrant Heritage of America Series.) Boston: Twayne Publishers. 1975. Pp. 326. \$9.95.

Gerald F. De Jong's *The Dutch in America, 1609-1974* is a straightforward history. De Jong opens his narrative by discussing the context of the early Dutch migration to America and by launching Henry Hudson—although not a Dutchman—on a round-about voyage to the river now bearing his name. After getting the Dutch onto American soil, De Jong assesses their place in the land. Underlying the narrative are traits identified with the Dutch experience in America: clannishness, conservatism, individualism, morality, piety, and thrift. Those traits were particularly characteristic of Dutch-Americans before World War II and the Dutch loss of Indonesia. Noting the effect of the loss of the Asian colony on immigration patterns, De Jong points out that Dutch Indonesians have favored California as a place to settle. Interestingly, that preference is also true of the more recent Dutch immigrants from Europe. California's Dutch population reached 27,333 in 1970 as compared with 9,754 in 1940. Meanwhile, Mich-

igan's foreign-born Dutch population shrank from 24,722 in 1940 to 15,095 in 1970. In short, the figures illustrate the migrant's urge to find a road to what he considers a more promising future.

Along with upheavals caused by World War II, the rapid expansion of communication and transportation has helped to diminish the purity of Dutch culture in America. Americanization of Dutch migrants has accelerated, with older customs surrendering to the new. On the other hand, as De Jong indicates, ease of air travel has enabled Dutch Americans visiting Holland to re-evaluate their European backgrounds.

De Jong closes his book with a chapter cataloging a century of Dutch contributions to American life in culture, science, and technology. Among the scientists De Jong mentions is Gerard Troost, whose career reads like a romance and whose geological reports of early Tennessee possess both charm and color. Troost's geological collection is housed at Louisville, Kentucky, and his notes concerning it are well worth reading.

Altogether, De Jong has produced a substantial work supported by both a substantial bibliography and statistical tables. The index is adequate, but there are no maps locating Dutch settlements in America. There is one picture showing a view of early New Amsterdam with a lone windmill in the background. De Jong's study might have gained a dimension from additional pictures; for example, illustrations of Dutch architecture, festivals, and personages. Indeed, while preparing this review, the writer saw a photograph marking Father Van den Broek's career in the Lower Fox River Valley, and it seemed to give a certain vibrancy to the printed treatment of the priest's work.

De Jong has organized his study skillfully, and he has made his points precisely. The only technical flaw detected by the reviewer is the statement that a De Pere, Wisconsin newspaper "still had a circulation of 1,250 in 1952, and did not cease publication until 1919." There is also an epilogue in which he cautions that his book has made no attempt "to exaggerate the role of the Dutch in American life." "In short," he concludes, "the Dutch-Americans have had about the same hopes and aspirations that have . . . motivated most people, regardless of their ethnic background."

EDWARD NOYES

*University of Wisconsin—Oshkosh*

THOMAS F. ARCHDEACON. *New York City, 1664-1710: Conquest and Change*. Ithaca: Cornell University Press. 1976. Pp. 197. \$9.75.

This book provides fresh insights on social structure and political behavior in New York City in the

half century after 1664, when the Dutch community was adjusting to the effects of the British takeover. It benefits from the author's imaginative mining of Common Council minutes, tax lists, and customs records and his interpretation of the data by using biographical data he assembled on a very sizable proportion of the heads of families listed on the assessment rolls. Interpreting assessment lists published in the council minutes for 1677, he concludes that the city was then at least eighty percent Dutch and that the Dutch were dominant in each of a series of intervals of wealth.

The tax rolls of 1703, however, reveal a city in which, though the Dutch still constituted close to sixty percent of the white population, residents of English and French background (English about twenty-nine percent; French eleven percent) had a disproportionately large presence in the ranks of New York's most affluent and influential citizenry. They outnumbered the Dutch two to one as merchants and constituted a majority of the inhabitants in the wealthiest ward, whereas Dutch accounted for four out of five families in the poorest ward. French Huguenots made common cause with the English because of the welcome the English gave them and because Huguenot church organization was more like that of the Church of England than that of the Dutch Reformed Church.

Thomas F. Archdeacon argues from these findings that New York became "an English city during the last quarter of the seventeenth century" and that support for the Leisler Rebellion can be laid to Dutch resentment over the "loss of power to the new local order—the English and those Dutch who had accommodated themselves to their rule." When New York became "an English city" is perhaps a semantic consideration. Foreign travelers described New York as "predominantly Dutch" into the early eighteenth century, and residents of Dutch background had to be reckoned with in local politics for some time after 1700. However, his conclusion that ethnic factors figured significantly in political contests is supported by quantitative findings based on a "scrutiny" of votes in a disputed municipal election reported in the council minutes of 1701. Judging from this presumably trustworthy evidence, nationality background "was the most important factor distinguishing the Leislerians from the Anti-Leislerians."

BAYRD STILL

*New York University*

JON C. TEAFORD. *The Municipal Revolution in America: Origins of Modern Urban Government, 1650-1825*. Chicago: University of Chicago Press. 1975. Pp. viii, 152. \$9.75.



This slender monograph expertly describes the "municipal revolution" in urban America from the early colonial period to the eve of the Jacksonian movement. Jon C. Teaford's narrative illustrates the manner in which the European-style closed municipal corporation failed to survive the liberating influence of the American environment. Thus Teaford's "revolution" altered the American urban condition from one where "questions of quality, quantity, price and vocation" were matters of importance for local government to the triumph of the doctrines of *laissez faire* and democracy. The democratic tide that emerged during the eighteenth century overwhelmed the attempts by the colonial oligarchs to maintain tight and restrictive controls over life in the cities.

Beginning in the early eighteenth century, reformist demands slowly but inexorably undercut the restricted access to the trades and to the ballot. The democratic spirit of the century reached a climax during the 1770s. In the wake of the American Revolution, equalitarian-bent state legislatures intervened to destroy local monopoly and to enable the urban majority to grasp the reins of government. The result was that by the 1830s a new order had been established in urban America, one dedicated to broad political participation and the destruction of municipally imposed trade monopolies.

Teaford's book will be useful to urban historians and to political scientists concerned with the origins of American urban government. The study is based upon extensive research into primary materials, and it reflects a judicious use of secondary literature. Teaford provides sufficient examples to make his points, but he is wise enough to refrain from overwhelming his reader with excessive detail. Written in a workmanlike manner, it should stand the test of time and remain for many years the authoritative study of this important, if unglamorous, aspect of the American urban heritage.

RICHARD O. DAVIES  
*Northern Arizona University*

JAMES LANG. *Conquest and Commerce: Spain and England in the Americas*. (Studies in Social Discontinuity.) New York: Academic Press, 1975. Pp. vii, 261. \$12.95.

This is an unusual book, a work of comparative history by a young historical sociologist, yet a strangely old-fashioned study. The main things compared are economic and bureaucratic arrangements; no exotic concepts, terminology, or methods are employed, and there is no pretense of primary research.

The book is divided into two parts of nearly equal length, "Spain in America" and "England

in America," followed by a sixteen-page interpretive conclusion. The emphasis throughout is strongly economic: "State power was closely associated with commercial hegemony." Spaniards understood this relationships as well as other Europeans, but their pre-Reformation entry into empire-building and their success in locating easily exploitable resources led them into a self-defeating monopolistic policy.

Despite major policy mistakes, economic development went forward in the Spanish colonies. "Our image of colonial Spanish America as a stagnant mass capable only of silver production is false." Instead, "the economic interests and needs of the colonists in the long run determined the nature of the economy," which prospered until the end. Consequently, Lang argues, "the present pattern of economic underdevelopment and dependency, characteristic of Spanish America, is not inherent in the colonial situation of the seventeenth and eighteenth centuries."

What the Creoles failed to dominate was the political structure of the empire. The crown valued "the normative order of Christ and King" above any legalistic or economic order, and through judicious concessions and "the Hapsburg system of competing jurisdictions" it was long able to retain the loyalty of the bureaucracy.

In general, the opposite occurred in English America. Here the parent state was successful in establishing control over the imperial economy, largely because it focused so completely on that task. Founded mainly by businessmen, "English America was a business." Fashioned largely by businessmen in parliament and the Board of Trade, England's colonial policy was "an enormous success both commercially and strategically."

Yet this success was bought at the cost of conceding effective control over the political order to the colonists. Concerned "to balance private interest and public welfare," English authorities found it expedient to create assemblies as a check on profit-mongering. In America this old joint-stock company device was transformed into "a focus of social integration" capable of coping with political crises, including the ultimate crisis of revolution. With such a focus lacking in Spanish America, "the independence movement rapidly assumed the character of a civil war."

It is easy to fault this work for the narrowness of its approach, in particular its low sensitivity to ethnic interaction. Thus, Spanish America was not simply "the creation" of pre-Reformation Spain, Indians in Spanish America were not simply "exploitable resources," and their cultures did not simply "devolve" into insignificance. Lang has, however, produced a well-written, persuasive, and



sometimes provocative synthesis of recent scholarship on the structure of the two empires. This is a considerable achievement, and students of both empires will find it suggestive.

JOHN T. JURICEK  
Emory University

ELIZABETH A. H. JOHN. *Storms Brewed in Other Men's Worlds: The Confrontation of Indians, Spanish, and French in the Southwest, 1540-1795*. College Station: Texas A & M University Press. 1975. Pp. xvi, 805. \$18.50.

This is an impressive book with greater potential for enduring value to the history of the Southwest than anything that has been published for over a decade. Its most significant contribution, and the quality that makes it a landmark work, is the enormous detail that Elizabeth A. H. John has indefatigably gleaned and reported on from the Spanish archives of New Mexico and the Bexar archives of Texas. This alone will change some earlier interpretations of Southwestern history, enrich and enhance others, and guide those of future historians. No one will ever be able to work in this area again without constant reference to *Storms Brewed in Other Men's Worlds*.

This achievement required a dual effort: the reading of the difficult manuscripts of seventeenth- and eighteenth-century imperfect Spanish as well as of a substantial body of ancillary material to isolate the factors that affected Spanish-Indian relations; and second, the organization of these data into a reasonably coherent narrative.

The first part of this task was excellently done. John seems to have examined all of the material in both major archival groups and extracted everything related to Indians. To her credit, and an incidental benefit, she has cited the work of other scholars, especially translated and published sources where it was feasible. She has also utilized the syntheses of previous major works about the Hispanic Southwest. A weakness in the work is that it is almost exclusively manufactured from administrative documents. Relatively few accounts of the padres or records of their missions are used, primarily because these are for the most part not accessible, even to so serious a scholar as John. The lack is noticeable, however, when even published and translated accounts, such as the journal of Father José de Solís, are not mentioned, and the accounts of Father Juan Agustín Morfi are accorded but two slight references.

The second task, producing a coherent organization, was as difficult as the first, but in this the book does not succeed quite as well. It is somewhat laborious to read. The sources tell the story: where evidence is ample the tale is lengthy; where evi-

dence is scanty the yarn is skimpy. Chronology shifts back and forth, sometimes confusingly, as John shifts focus to New Mexico, then Texas, then the Interior Provinces, and from one Indian group to another. But to organize such a wealth of material in a way that would satisfy one reviewer was a formidable task, and on the whole the book may have more lasting value because John did not impose too much organization on the material.

The book is not well titled. It is specifically about Spanish-Indian relations in Texas and New Mexico and is based on Spanish administrative sources. It is an objective recital in 766 pages of what took place, done in the finest scholarly tradition. The title, and a five-page "Afterword," seem to be a sop to the sententious romanticism currently in vogue.

SEYMOUR V. CONNOR  
Texas Tech University

CLIFFORD K. SHIPTON. *Sibley's Harvard Graduates. Volume 17, Biographical Sketches of Those Who Attended Harvard College in the Classes, 1768-1771*. Boston: Massachusetts Historical Society. 1975. Pp. x, 724. \$25.00.

This rich and delightful volume concludes a series of fourteen, produced as a labor of craftsmanship and love by Clifford Kenyon Shipton (1902-73), archivist, antiquarian, and *raconteur extraordinaire*. In 1930 Shipton, then a Harvard graduate student, assumed the long-dormant project inaugurated and subsequently endowed by the Harvard librarian John Langdon Sibley. During more than four decades Shipton chronicled the lives, achievements, and eccentricities of eighty-two Harvard classes, from the era of Sir Edmund Andros to the eve of the American Revolution. "Sibley-Shipton" now awaits an appropriate successor.

Particularly fascinating in this final opus is the juxtaposition of important political and cultural leaders in the new nation with obscure provincial figures, who are sketched with the same care and incisiveness: patriarchal pastors, local officeholders, merchants and schoolmasters, adventurers and rogues. Here is an unusually large group of alumni—192 in all; the class of 1771 was the largest in Harvard's history to that date. About one-third entered the ministry; a significant number filled frontier pulpits in Maine and New Hampshire. (Shipton records that almost all suffered from Revolutionary War inflation!) Among the clerics, Shipton's sketches of Edward Sprague, village eccentric, Stephen Peabody, diarist and schoolmaster, and David Osgood, Federalist polemicist, are particularly memorable. "Scales the Shaker" (William Scales, class of 1771) was truly extraordinary. One-sixth of the graduates were lawyers, including Theophilus Parsons, who was a leader of

the New England bar, and Perez Morton, who was attorney general of Massachusetts. Another sixth were physicians.

The theme of conflict runs through this volume: an undergraduate protest by the class of 1770 was the result of a confrontation with a tutor; the Whig-Tory division produced local feuds in New England communities; there were the hazards of the Revolutionary War itself and the postwar controversies between Federalists and Republicans, Orthodox Congregationalists and Unitarians. Some graduates lost fortunes or met an early death. The experiences of the loyalists were diverse: some subsequently returned to New England, others became leaders in New Brunswick or Nova Scotia, some prospered in Britain. Most poignant was the death of Governor Hutchinson's son Billy. For others the Revolutionary era was a time of great opportunity. Samuel Phillips, whose mills produced gunpowder for colonial troops, founded an academy in his native Andover. James Bowdoin, Jr. endowed a college in Maine. Winthrop Sargent became territorial governor of Mississippi. Others joined land speculation schemes in Ohio. After medical service in the war, John Warren was more responsible "than any one man" for changing "the New England concept of a physician as a pedestrian hack." Shipton portrays as well recipients of honorary or *ad eundem* degrees, including Timothy Dwight of Yale and the Marquis de Lafayette, whose New England associations are detailed.

Whither *Sibley* after Shipton? Lawrence Stone has argued persuasively for a more systematic prosopography. David Potts plans a central archive of biographical data on students. The new social history flourishes, and collegiate populations offer an important resource for analysis and generalization. A topical index to the entire *Sibley* series is badly needed. Although there is no consensus on the objectives of educational history, it remains a promising and undercultivated field at a time when educational sociology prospers. The study of education in America—whether of individuals or of groups—will be seriously distorted if the humanistic approach is subordinated. Shipton's contribution has been to provide, through a unique blending of massive erudition and crisp, even-handed exposition, a far more subtle and precise calibration of social roles than any model builder has yet achieved. Thus he has offered a convincing demonstration of the essentially humanistic foundations of sociology itself.

THEODORE R. CRANE  
University of Denver

GORDON E. KERSHAW. "*Gentlemen of Large Property & Judicious Men*"; *The Kennebeck Proprietors, 1749-1775*.

Somersworth: New Hampshire Publishing Company for the Maine Historical Society. 1975. Pp. xvi, 343. Cloth \$15.00, paper \$6.95.

To the number of useful studies of colonial American land companies can now be added Gordon Kershaw's book. Building on solid research, he describes how members of the Massachusetts elite organized the Kennebeck Purchase Company to augment their wealth by land speculation, as did their counterparts in other colonies. The book traces the history of the land, the company, and the settlement, but its main function is to portray Dr. Silvester Gardiner, James Bowdoin, and other prominent speculators in the Bay Colony. Kershaw discusses their backgrounds, their participation in the company, and their involvement in the pre-Revolutionary crises in Massachusetts. The author's discussion of the proprietors extends beyond their company activities, but he infrequently brings the Indian inhabitants and the settlers on the central Maine lands into the story; Kershaw's concern is the elite leadership.

The organization of the company, the financial arrangements, the returns to investors, the efforts to gain government support (by gifts to two Massachusetts governors), and the establishment of settlers on the patent are analyzed and interpreted convincingly and vigorously. Kershaw is less substantial and less interesting when linking the company, the proprietors, and pre-Revolutionary events. The controversy over the reservation of masts, insofar as the company was concerned, apparently was not "closely interwoven" with the revolutionary issues (p. 223). The opposition to Anglicanism that helped to fire revolutionary ardor in Boston played little part in the controversy between the Anglican missionary installed by Gardiner and the Congregationalist settlers. The case of John Hancock seems to demonstrate that, for the proprietors, the company became less important as the imperial crisis heightened (p. 84).

Kershaw handles well the significant question of whether the Kennebeck Company helped to instigate the attack by the Boston mob on Thomas Hutchinson's house. He finds connections between the Loyal Nine and the proprietors, but he notes that the evidence is inconclusive. The character of the proprietors' activities suggests that they might have stooped to inciting violence to cover the theft of evidence against them, but the character of the Bostonians suggests that the mob needed no ulterior motive.

As an important and informative contribution to scholarship, this book deserves better proofreading and clearer maps.

BENJAMIN H. NEWCOMB  
Texas Tech University

H. G. NICHOLAS. *The United States and Britain*. (The United States in the World: Foreign Perspectives.) Chicago: University of Chicago Press. 1975. Pp. viii, 195. \$10.00.

H. G. Nicholas approaches the history of Anglo-American relations from 1776 to the present from the viewpoint of a scholar long experienced in the study of British and international politics. His knowledge of American politics, too, is extensive. The text is salted with arresting (if anachronistic) cross-references, such as his characterization of the events of 1775-1781 as the "loss of America," and of George III's concern about the consequences for the West Indies and Ireland as an early instance of the "domino theory."

This use of foreign policy as the prism through which America and Britain have often seen each other may be said to be Nicholas' distinctive contribution. What particular insights does his method make possible? One result is an unusually clear perception of the common "Anglo-American" or "paranational" outlook which the United States and the United Kingdom have shared in world affairs. Because of their geographically insular situations, he points out, both countries have been able to exercise exceptional freedom of choice in their international relations. A concomitant of this, he argues, is "a pervasive disposition to judge issues of foreign, as well as domestic, policy by moral criteria" (p. 1). If this common moralistic outlook has led to the adoption of similar positions vis-à-vis other nations, it has also produced a certain captiousness in their bilateral relations. The protagonists of what he calls the "family quarrel" have shown "an uncanny skill in knowing where the moral stiletto can be inserted to best effect" (p. 4). Another consequence of this shared moralism has been a tendency to overlook or incorrectly gauge the element of power in the Anglo-American relationship. Time and again—the War of 1812, the Civil War, the Venezuelan crisis, the First World War, the Second World War, Suez, the Cuban missile crisis—Washington and London have badly misjudged, initially at least, the degree of their mutual vulnerability and dependency.

As an extended essay rather than a textbook or monograph, *The United States and Britain* does not purport to provide complete or new factual information. It does, however, reflect wide reading in both primary and the best secondary sources, and it is generally accurate. Most of the historical judgments in the book, too, are unarguable, except for occasional exaggerations, for example, the author's observation that Franklin Roosevelt, in opposing British postwar plans in Asia, saw "Western imperialism as more of a threat than Stalinist communism" (p. 134). On post-World War II de-

fense matters, a subject to which American historians have as yet given little attention, Nicholas is both cogent and stimulating.

ALAN K. HENRIKSON

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Tufts University

ARTHUR H. SHAFFER. *The Politics of History: Writing the History of the American Revolution, 1783-1815*. Chicago: Precedent Publishing. 1975. Pp. 228. \$12.50.

This book, which deals with American historians of the first national generation who wrote about the Revolution, addresses itself to a historiographical topic that has long been slighted. Because of their strident patriotism, undue emphasis on national unity, and doctrinaire didacticism, their writings have often been dismissed as inferior or too biased to be of much value. Arthur Shaffer has shown that this generation of some forty-odd authors, who included in their ranks David Ramsay, Jeremy Belknap, Mercy Otis Warren, John Marshall, and Edmund Randolph, made a significant contribution by reshaping American historical thinking in the light of the experiences of the Revolution and of nationhood. Although there existed among Americans a sense of uniqueness from early Puritan times, these writers translated it into a nationalist doctrine that has been an important part of America's historical consciousness ever since.

Shaffer approaches historiography from the perspective of intellectual history and treats his subject topically with themes like views of human nature, environmentalism, and national character, instead of taking up one historian after another. He seeks also to relate these writings to the Enlightenment philosophy of history current at the time in Europe and America, arguing that the American view was both the same and different. The dissimilarities arose from the different way in which Americans applied the principles of the Enlightenment ideal of history—such as the universality of human experience—because of their preoccupation with a distinctively national experience. This brief summary admittedly fails to do justice to the rich treatment given to these and other themes.

If there is any criticism to be leveled at this valuable and thoughtful study, it is of Shaffer's de-emphasis of religion. Although he has taken pains to show that few Americans fully embraced Enlightenment skepticism, Shaffer argues that most writers postulated a vague providential conception of history that represented a secularized version of the seventeenth-century Puritan vision of a "city upon a hill." He takes this position because his view of the Enlightenment in America

is essentially that of the secular European Enlightenment with some qualifications. It may be argued instead that the American Enlightenment was infused with powerful religious and millennial ideas, and that secular-minded, twentieth-century historians should account much more for the influence of Puritanism, reformed Protestantism, and Pietism in the post-Revolutionary period than they have done in the past.

GEORGE ATHAN BILLIAS  
*Clark University*

CLAUDE-ANNE LOPEZ and EUGENIA W. HERBERT. *The Private Franklin: The Man and His Family*. New York: W. W. Norton and Company. 1975. Pp. xv, 361. \$11.95.

The authors of this interesting, readable book do a good job; they write in a lighthearted style. Short chapters often center on one focal theme and are written as vignettes capable of standing alone. The danger of such an approach, however, is that the finished book may be disjointed. Herbert and Lopez avoid this pitfall, and the reader comes back to the book again and again. All those who belong to the "family" may be found here somewhere: Debbie, William, Sally, Francis Folger, Temple, Benny, Richard Bache, Polly and Margaret Stevenson, Jonathan Williams, Jr., Aunt Jane Mecom—even Theophile. Through and over the story towers the person of Benjamin Franklin.

More successfully than anyone else, the two authors have made those in Franklin's circle real people. The description of Sally Franklin is excellent, and the portrayal of Jane Mecom's tragic life is perhaps the best thing in the book. Throughout their story, Lopez and Herbert make plain the ordeal of death that so repeatedly ravaged most colonial families and the stoicism survivors had to adopt in order to endure.

For the first time, we can read of Franklin's long-suffering wife as a person in her own right. Usually depicted as slow, dumb, and dowdy (the better to explain why Franklin's eyes were usually on someone else!), Deborah is described in *The Private Franklin* as self-possessed, self-reliant, tough-minded, calm under pressure, and with desires and ambitions of her own (which makes Franklin's treatment of her less understandable). A remarkable lady, Debbie is worth every word the authors have spent on her.

There are some shortcomings in this work. Chapters wander, covering too much and saying too little; directionless, as if the authors were unsure whether to focus on people or to follow the usual biographical path of recounting Franklin's achievements and interests. Other recent writers,

such as Buxbaum, Newcomb, and others, do a much better job of analyzing many of Franklin's motives and seem to have more insight into his relationships with politicians, comrades, partners. Some of the authors' interpretations are inadequate, and some neglected sources would have been helpful to them. Intricacies of land speculation, for example, are summarized to the point of inaccuracy.

The Franklin who emerges from these pages is often petulant, occasionally tyrannical with a terrible temper. The writers show the pettiness he demonstrated for years toward his only daughter, Sally, because of her desire to marry Richard Bache. His egocentricity shines through all his activities. We glimpse his idle regrets that he was so much and so long absent from Debbie and Sally, but we realize also that he had no strong feelings toward them. The dust jacket sums up much that the authors discuss inside: that Franklin ignored pleas from his wife of nearly forty years that he return to her when she was dying, renouncing William, using Temple, neglecting Benny; reserving affection from those to whom he was father, husband, or family while pouring it out upon others who better suited him; and that he was "extraordinarily insensitive to his immediate family." The authors' depiction, in spite of their obvious liking for Franklin, is not a pretty one. It does add to our understanding of this pivotal figure.

CECIL B. CURREY  
*University of South Florida*

WILLIAM B. WILLCOX, editor. *The Papers of Benjamin Franklin*. Volume 18, *January 1 through December 31, 1771*. New Haven: Yale University Press, sponsored by the American Philosophical Society. 1974. Pp. xxix, 302. \$20.00.

This volume contains important political letters and documents (such as Franklin's interview with Lord Hillsborough), charming letters (like those to "Polly" [Stevenson] Hewson), scientific observations and speculations (such as a discussion of raindrops, with notes on what constitutes "a thoroughly satisfactory [scientific] hypothesis"), and moral philosophy (for example, on the nature of "the Love of Money"). It is surprising that it omits part 1 of the autobiography, which Franklin wrote between July 31 and August 13, 1771. The editors do not explain the omission. Perhaps they believe that the 1964 edition by Leonard W. Labaree *et al.* precludes the necessity for the autobiography's being included in the *Papers*. But in the 1964 edition we read that "the editors will present a more detailed discussion of the autobiography's bibliographical history and of their views on some of the



conflicting points of evidence at the appropriate place in a future volume of *The Papers of Benjamin Franklin*" (p. 27). Where, if not in the appropriate chronological volumes, can one expect to find such a discussion? And where can one expect to find the text of the *Autobiography*? The only possibility now seems to be a separate volume, published after the appearance of the chronological series, but what I really suspect is that William B. Willcox does not intend to print Franklin's masterpiece in this monumental edition.

The headnotes, annotations, and index are generally excellent. The classical quotations, however, should be translated in the footnotes, and the identification should be specific, not simply "Terence" (cf. p. 217). To be useful, the long index entry under "Franklin, Benjamin" should be subdivided topically. But these are quibbles. I find four drastic methodological faults with the edition.

First, the reader is told, time and again, that another authoritative copy of a piece exists and that the editors are noting any "important variations." The standard procedure, not followed in this edition, is to list all substantive differences and to omit all accidentals, unless the accidentals affect meaning. Second, Willcox sometimes prints from secondary sources when the original exists and is available. Thus he perpetuates any errors that have been introduced into his copy. Third, Willcox inaugurated the practice of printing resums of letters to Franklin and even of letters by Franklin. That means that Willcox has deliberately chosen to publish an incomplete edition of the writings of Franklin. And fourth, Willcox frequently advocates interpretations of Franklin's character, politics, and even of his literary ability. He does this not only in the introduction, but also in headnotes and even in the footnotes. In this volume he gratuitously comments about Franklin's lack of sympathy for the financial shenanigans of Deborah Franklin. When Willcox hindsightedly calls her "old and failing" (p. 90), one might almost suppose that he was speaking of Franklin's mother! As in the preceding volumes, Willcox represents Franklin as a trimmer in politics, and he ignores the many ways in which Franklin had been and was more radical than almost any of his contemporaries. When Willcox encounters contrary evidence, he even suggests that Franklin may have falsified it to gain favor with the American radicals (pp. 11-12). Although Willcox refutes the absurdities of Cecil B. Currey (p. 88), he is, in some ways, even more reprehensible than the obviously distorted Currey, for he insidiously surrounds the facts and documents that he presents with his own interpretation. My point is not simply that his interpretations are wrong, although they often are;

it is that interpretations do not belong in a standard edition.

With the exception of the first fault, these errors are Willcox's innovations. It is, I hope, not too late to redeem the edition. Even under Willcox, *The Papers of Benjamin Franklin* is usually a great edition; at its worst, it is a deliberately selected edition, rather than a complete one, of unsound texts, full of officious editorializing.

J. A. LEO LEMAY  
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Los Angeles

RICHARD B. MORRIS, editor. *John Jay: The Making of a Revolutionary*. Volume 1, *Unpublished Papers, 1745-1780*. New York: Harper and Row. 1975. Pp. x, 866. \$27.50.

This is a magnificent book edited by Richard B. Morris, his associate Floyd M. Shumway, and their assistants Ene Sirvet and Elaine G. Brown. It has always been difficult for me to like John Jay, the conservative or, at best, fence-sitter, so unlike Christopher Gadsden, the hotheaded advocate of besieging Boston and declaring independence. But a careful examination of Jay in this documentary biography indicates a judicious man who moved slowly toward right conviction. He hated Catholics; yet he contributed mightily toward separation of church and state and then performed effectively under the most trying conditions as American minister to the Catholic court of Spain. He learned quickly, as did the other ministers abroad, that to trust no one was the chief virtue of the good diplomat.

In Spain his correspondence with friends and associates indicates what great suffering the proud John Jay endured. Not only did his country embarrass him with its indebtedness, but Congress enthusiastically pursued expansionist goals and often imposed unrealistic demands on its ministers. This was especially true in regard to Spain.

Jay's personal sacrifices were legion. In his travels he was forced to stay in Spanish inns where lice, vermin, and avaricious inn-keepers were not uncommon. The Jays' delight at the birth of their child and then the pain of the baby's death, within a month, are fully recorded. Sarah Jay is revealed in these letters as one of the great eighteenth-century ladies, ranking with Abigail Adams and Mrs. Pinckney.

The methodology enhances the documents. Every item is well researched, and the headnotes make this a readable documentary biography, a feat not easily accomplished in such a work. I am puzzled, however, by the exclusion of a table of contents listing the documents.



There was a great need for a new edition of Jay's papers. Compare, for example, Henry P. Johnston's, *Correspondence and Public Papers of John Jay* (vol. 1, pp. 43-45) with pages 231-33 of Morris' work. Johnston leaves out important parts of the letter in question, usually social history, and he dutifully punctuates in accordance with nineteenth-century rules of grammar. There is also much new material in Morris' edition that local, state, national, and international historians will find invaluable.

RICHARD WALSH  
Georgetown University

HERBERT A. JOHNSON, editor. *The Papers of John Marshall*. Volume 1, *Correspondence and Papers, November 10, 1775-June 23, 1788; Account Book, September 1783-June 1788*. (Sponsored by the College of William and Mary and the Institute of Early American History and Culture, under the auspices of the National Historical Publications Commission.) Chapel Hill: University of North Carolina Press. 1974. Pp. xlvii, 448. \$17.95.

Since 1906 American scholars have talked of publishing the papers of Chief Justice John Marshall, but earlier editorial projects succumbed to the difficulties inherent in accumulating the necessary materials. Marshall was unconcerned about posthumous fame and left behind no sizable collection of personal or professional correspondence. Hence, would-be editors have been forced to engage in Holmesian feats of detection by ransacking distant archives, tracking down family descendants, and scrutinizing auction catalogs in search of elusive Marshall documents. The bicentennial celebration of the chief justice's birth in 1955 intensified scholarly interest in a definitive edition of his writings and led to a major new publishing venture. Sponsored by the College of William and Mary and the Institute of Early American History and Culture, in collaboration with the National Historical Publications Commission, the *Papers of John Marshall* are scheduled to appear in approximately ten volumes. In this initial, and long-awaited, volume some of the potential strengths and weaknesses of the series may be seen.

The editors assert that "every extant Marshall paper from the time of his birth to June 30, 1788, is printed or calendared in this volume" (p. xxvi). More than three-fourths of the assembled documents have not been published previously, although scholars have made use of the manuscripts in many cases. Measured by any standard of comprehensiveness, the editors have done a superlative job. Their research is meticulous, their organizational plan sensible, and their interpretive commentary clear and enlightening. But the canon of

inclusiveness, once adopted as a basic operating principle, raises questions of relative merit and emphasis that are not easily disposed of. While the editors have not hesitated to summarize many items of negligible importance, they have not entirely escaped the temptation of publishing at length other documents whose historical value appears equally slight.

Certainly one cannot quibble with the decision to publish accounts of Marshall's early cases before the Virginia General Court, drawn from the casebook of St. George Tucker. Through the skillful editing of this material and related items, one gains a real awareness of the nature and extent of Marshall's legal practice, as well as the general level of professional performance in the higher courts of Virginia during the 1780s. Similarly helpful are the treasury warrants, deeds, and correspondence pertaining to Marshall's land speculations and the reprinting of his speeches in defense of the Constitution at the Virginia ratifying convention.

But in other areas, the editors should have wielded their blue pencils with greater ruthlessness. An extract of forty-three pages from Marshall's derivative law notes is excessive, as is the publication of eight complete muster rolls or payrolls of the Continental Army. And the repetitive personal items in Marshall's account book add nothing to Albert Beveridge's standard treatment of the man, which utilized the same source to good effect fifty years ago.

MAXWELL BLOOMFIELD  
Catholic University of America

E. JAMES FERGUSON, editor. *The Papers of Robert Morris, 1781-1784*. Volume 2, *August-September 1781*. Pittsburgh: University of Pittsburgh Press. 1975. Pp. xxxv, 400. \$20.00.

During the period covered in this second of a projected twelve-volume set, Robert Morris was already involved in some of the most important fiscal issues that emerged during his tenure as American superintendent of finance. In both August and September, proposals that the armed forces be supplied through private firms, rather than through government-administered requisitions, were tested as contracts were advertised and granted for the supply of sundry installations and troops in Pennsylvania. A complicated shuffling of funds was simultaneously undertaken toward the goal of paying a month's salary to the forces marching with Major General Benjamin Lincoln toward Yorktown. Finally, during September, operations were begun that were designed to bring to the treasury in Philadelphia a cargo of French

specie that had recently been received in the port at Boston.

As E. James Ferguson indicated in *The Power of the Purse* (1961), reductions in the size of the American army and the increasing availability of foreign aid and loans, among other factors, had by 1781 simplified the task of financing the revolutionary effort. Continuing difficulties of communications, recalcitrant state governments, and periodic threats of British raids still beset those who sought to administer the nation's fiscal programs. The items contained in the current volume indicate that Robert Morris was an active and forceful figure whose enterprise and ingenuity played a major role in the fiscal successes of the year.

Much of the correspondence also documents the growing influence Morris was acquiring in an ever-wider range of governmental activities. During these two months, for example, Morris' view regarding a proposed reorganization of the military hospitals was solicited, and he was given control over the functions previously performed by the Board of Admiralty and the various navy boards.

Here, as in the first volume published in this series, the editorial effort is of high quality. While avoiding extended essays on the actions and philosophies evident in the documents, the work contains many biographical and informational notes and a useful index.

There are some continuing problems. By design, this set has been restricted to the public papers of Robert Morris, including the official diary of the office of finance, and letters passing to and from that office. The frequency of the notation "letter not found" testifies to the enormous difficulty in attempting to present even those documents referred to in the official finance diary.

On balance, however, this is a valuable work, not only for specialists concerned with the financial history of the period, but for others seeking a sense of some of the difficulties that plagued those who administered the fight for American independence.

BRUCE M. WILKENFELD  
Hunter College,  
City University of New York

ROBERT A. RUTLAND, editor-in-chief. *The Papers of James Madison*. Volume 9, 9 April 1786–24 May 1787, with supplement 1781–1784. Edited by WILLIAM M. E. RACHAL. Chicago: University of Chicago Press. 1975. Pp. xxv, 447. \$18.50.

This curious collection of letters contains the correspondence primarily of national leaders who were disturbed by politics. It reflects the deepening crisis in the Confederation government, which was unable for a time to maintain even a

quorum in the legislature. Its miserable lack of power had shaken most of Madison's correspondents and was the single source of their anxiety. But Shays' Rebellion brought also a feeling of urgency, almost of impending disaster, and convinced them to back the Philadelphia Convention of 1787. Washington stated his feelings very well in these words: "No morn ever dawned more favourable than ours did—and no day was ever more clouded than the present!" Madison agreed, but his analysis of the crisis makes clear that he expected the people in their desperation to divide into several small political units or to seek monarchy as a solution for future disorders. "I hope the danger of it will rouse all the real friends of the Revolution to exert themselves in favor of such an organization of the Confederacy, as will perpetuate the Union, and redeem the honor of the Republican name."

With these expressions of crisis were others that seemed to contradict them. Jefferson felt a little rebellion was good for the political soul, while Madison prepared his mind for reform of the Confederation. His optimism for the triumph of popular government over its enemies was unshaken; he came to the Philadelphia Convention better prepared than any other delegate. Rutland says that what distinguished him was the blending of first-hand political experience with scholarly reflection, so that he was the best-informed man present at Philadelphia.

Rutland's estimate of Madison's ability is not readily apparent from these letters. What is evident is the presence of a devout patriot who wishes the best for his country. Alongside John Adams and Thomas Jefferson he appears less well rounded and less interested in the world of art and literature. His interests in land speculation and the natural order give some dimension; his family correspondence with his father occasionally gives insights of personality.

The editing of the volume is excellent; the notes are modest but informative; the essays by the editors are helpful in understanding the events of the day.

JOHN A. SCHUTZ  
University of Southern California

JULIAN P. BOYD, editor. *The Papers of Thomas Jefferson*. Volume 19, 24 January to 31 March 1791. Princeton: Princeton University Press. 1974. Pp. xxxvi, 646. \$22.50.

The major topics in this volume are the locating of the site for the Federal District; the political maneuvering precipitated by Benjamin Franklin's death; Jefferson's report on the fisheries; establishing the consular service; admission of Ken-

tucky and Vermont to the Union; confronting Spain on the Mississippi question; and Jefferson's attempts to gain domestic and international cooperation on an anti-British navigation law. Each of these episodes is placed by the editor in the context of the emerging party divisions. Julian P. Boyd repeatedly shows how these divisions were not the product of the French Revolution, but had roots in colonial history or in events of the 1780s.

In erudite and stimulating essays, modestly called editorial notes, Boyd gives the history, setting, and significance of each topical division. He recounts Washington's astute conduct in establishing the site of the Federal District, aided by Jefferson, an "acquiescent agent" (p. 58). Boyd argues that Jefferson's report on the fisheries, with its implications for a possible navigation act, was more significant in drawing party lines than was the bank bill. In his brilliant essay on the Mississippi question, Boyd weaves together pertinent information on land speculation, Indian policy, financial corruption, international finance, diplomatic history, and Jefferson's and Washington's concern to hold Western loyalties. Boyd discounts the Wilkinson-Brown "conspiracy," showing how Samuel F. Bemis and Arthur P. Whitaker were misled by their sources (pp. 471-74). He considers the reversal of Jay's Western policy as Jefferson's greatest triumph as secretary of state. Jefferson's varying roles are kept in focus throughout the volume's wide-ranging essays. In none of the episodes, it must be added, does Alexander Hamilton emerge with much credit.

In this volume we catch glimpses of Jefferson as family man, as scientist, and as the friend of archivists. At a time when his official duties were exceedingly heavy, Jefferson the scientist spent many hours demonstrating the worthlessness of Jacob Isaack's sea water desalination process. Jefferson endeared himself to historians and archivists when he stated briefly his concern to save and publish historical records: "The lost cannot be recovered; but let us save what remains . . . by such a multiplication of copies, as shall place them beyond the reach of accident" (p. 287). Jefferson's advice, of course, is a major justification for the publication of this extraordinarily useful edition of his papers.

MARVIN R. ZAHNISER  
*Ohio State University*

MARION BALDERSTON and DAVID SYRETT, edited and annotated by. *The Lost War: Letters from British Officers during the American Revolution*. Introduction by HENRY STEELE COMMAGER. New York: Horizon Press. 1975. Pp. xi, 237. \$11.95.

*The Lost War* is composed of letters from young British army and navy officers to their friend and

patron, Basil Feilding, Earl of Denbigh, which were discovered by Marion Balderston in the library of the home of the current earl in Warwickshire, and which have been published here for the first time. The letters have two focuses: the patronage system under which all the young men gained their commissions, and in which they had Feilding's help, and the progress of the American war. Thus they contain valuable information both for those studying the internal workings of the English military establishment in the eighteenth century and for those interested in British officers' perceptions of the Revolution.

It is curious that David Syrett's otherwise superb introduction slights the second of these concerns in favor of the first. He provides a fine working guide to the ins and outs of military patronage in the mid- to late eighteenth century in England, but he says little about the revealing nature of the officers' comments on the colonial situation. It is true that much of what is contained here tends to confirm what has been known about British officers' thinking rather than give new information: one learns again, for example, that the redcoats expected the South and the Middle Colonies to be swarming with loyalists, that they thought that the American army was composed largely of recent Irish and Scotch immigrants, and that they believed in 1776 that "this Summer, and next winter will totally destroy their Army, and the Misled deluded people will at last be convinced that they have been drawn into ruin, by a set of mock patriots" (p. 81).

But there are also new insights found in the letters. Not only do they document in significant detail Syrett's observation that "the government was looked upon by the heads of the great landed families of England as the employer of the last resort for their relatives and other dependents" (p. 21), but they also give us better and more complete information than was heretofore available on the conditions within the British army camps in America and the attitudes of junior officers thereon. And one wonders how many other British officers in late 1777, after the American victory at Saratoga, would have agreed with Major John Bowater when he wrote that "I every day curse Columbus and all the discoverers of this Diabolical Country" (p. 147).

MARY BETH NORTON  
*Cornell University*

JONATHAN GREGORY ROSSIE. *The Politics of Command in the American Revolution*. Syracuse: Syracuse University Press. 1975. Pp. xi, 252. \$12.50.

This volume is a curious analysis of certain aspects of rivalry among American military leaders dur-

ing the Revolution. Questionable emphasis is placed on the effects of the Horatio Gates-Philip Schuyler infighting and the Conway Cabal.

The author sets out to tarnish the shining armor of George Washington, terming him "unsuccessful"; but, with the exception of entertainingly pointing up a few of his petty faults already known, he fails. Washington still stands "like Pike's Peak among the lower mountains."

Helpful sources, including biographies, are cited, but not all have been sufficiently examined. The bibliography is limited. Some sources are quoted too frequently, and at times others are not footnoted. According to the book, "Philip Schuyler's continued procrastination eventually proved fatal to American ambitions in Canada." This is partly true, but the reason appears to be opposition from Gates and the Continental Congress. More decisive factors were the lack of men and supplies, Schuyler's arrogance, the consequent opposition of the New Englanders, the separateness of commands, the impetuosity of Benedict Arnold, and the untimely death of General Richard Montgomery in trying to capture Quebec.

General Gates is pictured as a harried and able leader. The weight of historical evidence, however, indicates he was a better adjutant than a general and allowed Arnold and Daniel Morgan to assume leadership of the Battle of Saratoga. At Camden, South Carolina, Gates lost the most disastrous battle in early American military history, fleeing on horseback from Cornwallis for sixty miles, then dashing rearward another sixty; this incident was not mentioned in the volume.

The Conway Cabal is played up, then minimized as if it had never existed. This irrational and jealous opposition to Washington by an Irish general from the French army, aided by Gates, caused Washington serious anxiety. But it was only one such tempest the commander in chief had to calm, with the help of such loyal officers as Henry Knox, Nathanael Greene, and Lord Stirling (whose name is often misspelled).

On the whole, this interesting book makes some contribution to our historical literature of the American Revolutionary War. But its significance is confined mainly to an auxiliary and comparatively minor phase of the war.

NORTH CALLAHAN  
New York University

JAMES LUNT. *John Burgoyne of Saratoga*. New York: Harcourt Brace Jovanovich. 1975. Pp. xiv, 369. \$14.95.

John Burgoyne has long held a fascination for military historians, perhaps because of his flamboyant character as well as his loss of a critical

battle. This study, using a biographical format, is an analysis of the Saratoga campaign of 1777. The author, a retired British major general, looks upon his subject basically as a military man, although Burgoyne's political and literary efforts come in for a fair share of attention. Lunt readily admits that he has uncovered no new sources of consequence, and he has based some of his conclusions on his own military experience. He speculates that Burgoyne's immediate descendants, conscious of their illegitimacy, destroyed their father's personal correspondence.

The picture of John Burgoyne as a politician that emerges in these pages is one of an opportunist who uses his parliamentary seat to further his military ambitions. As a military man he is pictured as a reformer, particularly in his treatment of the rank and file who affectionately bestowed upon him the name of "Gentleman Johnny." There is also the implication that Burgoyne was reluctant to use the Indians, but such a conclusion seems a bit out of character. A tendency to absolve the general from the responsibility of selecting German troops for the thrust against Bennington is also evident, but if he did not make that decision the responsibility for their failure must lie with him as commanding general. The decision to march overland rather than go by water is explained by the fact that this would have failed to reinforce the victory at Ticonderoga, the loyalists would have been disheartened, and it would have meant the abandonment of the threat of a strike against Connecticut.

That Burgoyne became the scapegoat of the affair was the result of the government's closing ranks against him lest an investigation reveal ministerial incompetence and bureaucratic bunnings. He never received the hearing that might have cleared him.

The author's battle accounts are lucid, graphic, and fast moving, and his interpretations are sometimes challenging. This is a good study, although in the light of some of the decisions made by Burgoyne, it is somewhat difficult to agree with the conclusion that he was unlucky rather than incompetent.

HUGH F. RANKIN  
Tulane University

FRANCES W. GREGORY. *Nathan Appleton: Merchant and Entrepreneur, 1779-1861*. Charlottesville: University Press of Virginia. 1976. Pp. xix, 358. \$12.50.

This excellent study is primarily for economic and business historians, but others interested in mercantile responses to diplomatic vicissitudes between 1794 and 1815 will find it valuable as well. Based on careful research in primary sources,



Frances Gregory's case for Nathan Appleton as the epitome of the New England merchant transformed into textile entrepreneur is a strong one. Appleton was one of the investors in the nation's first integrated textile mill at Waltham, Massachusetts, and his association with the industry continued for almost half a century—until his death in 1861. By then, he had invested \$800,000 in over thirty separate firms.

By his mid-thirties Appleton had already carved out a successful career as an all-purpose merchant involved in overseas trade. The son of a well-to-do New Hampshire farmer, Nathan joined his brother Samuel's mercantile firm as bookkeeper when the latter expanded his horizons from New Ipswich to Boston in 1794. The business prospered and they soon adopted a strategy which called for one brother to reside in England while the other remained in Boston. Thus positioned, they could coordinate more effectively their transatlantic shipments. Such cooperation was largely responsible for substantial profits on their ventures in the years prior to and during the War of 1812. Although these early chapters are plagued somewhat by repetitious accounts of overseas transactions, that format is fully justified since the author aims to show how the brothers reacted to sudden changes in the navigation regulations of the three rival powers.

After 1815 Appleton was among those merchants who gradually shifted their capital to manufacturing; indeed, no other biography reveals this transition quite so clearly. Appleton's major contribution was probably his innovative approach to marketing. The specialized textile selling agency, which he pioneered, not only assumed responsibility for distribution, but because of its continuous monitoring of sales patterns, it was often in a position to dictate the styles and quality of the cloth produced. Moreover, at a time when commercial banks shunned manufacturing firms, Appleton's sales agency made cash advances to the mills and thereby provided much of the working capital that kept them solvent.

Although much of the text was originally written over a quarter-century ago, it has been updated and shows few signs of obsolescence. It is difficult to understand how a study this fine could have remained unpublished for so long. Gregory plans to deal with Appleton's banking and political activities in a later volume.

EDWIN J. PERKINS  
*University of Southern California*

RANDOLPH SHIPLEY KLEIN. *Portrait of an Early American Family: The Shippens of Pennsylvania across Five Generations*. Philadelphia: University of Pennsylvania Press. 1975. Pp. ix, 373. \$14.00.

Historians should be grateful in these days of economic travail that university presses remain willing to publish doctoral dissertations that expand our knowledge of the past. The prose of Randolph S. Klein's 1972 Rutgers dissertation is, as with most theses, at best workable. And true to the genre, Klein makes greater claims to originality than may be warranted. "The traditional approach to historical figures as individuals," he writes, "often turns out to be a misleading preconception" (p. 6). In a later passage, he refers to "the error of looking upon Shippens simply as individuals" (p. 111). Klein berates an already outmoded historiography. Few scholars today, in the wake of the demographic explosion, not to speak of the researches of the Namier school, fail to take into account the centrality of family in eighteenth-century social, political, and economic life. Even before the new historiography, Leonard Labaree (*Conservatism in Early American History* [1948]) was only the most eminent of the many voices insisting upon the relevance of the kinship network to colonial politics. The familial approach has even percolated down to television, upon which numerous Adamsses have recently cavorted in their interconnected, though varying rolls.

Klein's secondary justification for this study, though similar to the explorer's "because-it's-there" rationale, is nevertheless perfectly adequate. There is a mother lode of Shippen materials that, before now, has never been adequately explored. Its exploitation within a kinship-network conceptual framework assuredly does, as Klein claims, "provide new insights into many perplexing problems of social history" (p. vii). Perhaps the most exciting of these insights is that concerning the vexing question of "how revolutionary was the American Revolution?" The fate of the fourth-generation Shippens suggests that "the revolution in Pennsylvania was more thoroughgoing than many care to admit" (p. 209).

Eight central chapters recount the history of the Shippen family, from Edward Shippen (1639-1712), the founder of the American dynasty, through the fifth generation; chapters 1 and 10 are, respectively, an introduction and a retrospection. Klein's ordinal designations for the many Edwards, Josephs, and Williams unerringly guide the reader through the thickets of Shippen genealogy. One hundred pages of footnotes include some of the author's sharpest insights. There are portraits of significant Shippens. Appendixes include Shippen vital statistics in tabular form and a schematic presentation of the family's kinship network. The extensive and suggestive bibliography includes the Shippen collections and an interdisciplinary variety of theoretical and historical works relevant to their story.



Two proofing errors and an illegible map of Pennsylvania in 1770 do not significantly mar an otherwise admirable production of an exhaustively researched and valuable story.

THOMAS WENDEL  
San José State University

JAMES R. MCGOVERN. *Yankee Family*. New Orleans: Polyanthos. 1975. Pp. iv, 191. \$15.00.

Study of the family, like cliometrics and other faddishly "in" approaches to history, has an inherent weakness. It is not that these approaches have no potential for enriching our understanding, as some skeptics are wont to charge; far from it. The trouble is that, to be worthwhile, they require an even greater measure of tough-mindedness, discipline, control of information, and imagination than does conventional scholarship, but they tend to attract soft-headed dullards seeking to use new methods as a substitute for intelligence and hard work.

It is therefore a pleasure to encounter James McGovern's *Yankee Family*, an intensive study of the Poor, Pierce, and Chandler families of New England through four generations, from the 1780s to the early twentieth century. McGovern was fortunate to have at his disposal an extremely full collection of family papers, including more than ten thousand letters, sixty diaries, and great quantities of clippings and miscellaneous memorabilia. What is more important, he has exploited the materials judiciously, blending common sense with a thorough command of the theories and concepts of a wide range of disciplines. Occasionally he allows the jargon of psychology and sociology to lure him into abstruse generalizations, but those occasions are admirably rare; normally he is the master of his tools. The result is a rewarding and readable study.

Such a work inevitably raises as many questions as it answers. The more or less central character in the story is Henry Varnum Poor, the long-time editor of the *American Railroad Journal* and subsequently the publisher of Poor's *Manual of Railroads*. He was certainly no ordinary Yankee. One wonders, in reading the excellent and often moving accounts of the Poores' marital relationship, their child-rearing practices, and their assignment of roles by sexes, just how typical was the family. Was their extreme polarization of sexual roles, with its inherent alienation, an increasingly common aspect of American family life in the mid-nineteenth century, or was it confined to the more successful professional and business men? Had it become the norm, before the Civil War, to turn the rearing and enculturation of children over to the mother, while the father earned his livelihood

apart from the family, or did most American families continue the traditional rural patterns? Were Yankee families drastically different from Southern families and from those of the Scotch-Irish frontiersmen?

McGovern believes his Yankee family was, in a sense, even more than typical of other New England families, since it combined two conflicting traditions that other scholars have observed, namely Puritanism and the frontier. He makes his case convincingly and claims no more. He also has a broader interpretive thesis that he follows consistently. Some readers may disagree with his conclusions and generalizations, but none should ignore them. Moreover, whether one agrees with his thesis or not, his work will stand for a long time as a rich storehouse of information.

FORREST MCDONALD  
University of Alabama

WILLIAM A. MURASKIN. *Middle-Class Blacks in a White Society: Prince Hall Freemasonry in America*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xi, 318. \$14.95.

The author states that "this work attempts to comprehend the black bourgeois experience by looking at one of its major institutions, Prince Hall Free Masonry" (p. x). He believes that his own "marginality" as a Jew gives him special insights into the subject, and he feels that E. Franklin Frazier was too preoccupied with the more elite members of the bourgeoisie and that Masons comprise a larger and more representative segment. Unlike Frazier, he considers the middle class "a tragic group more deserving of respect and empathy than condemnation"—a group "divided in its allegiance, . . . marginal to both the black and white racial communities" (p. 5).

William A. Muraskin has thoroughly combed available sources, and the book, which deals with Prince Hall Masonry from 1775 to the present, contains much fascinating material. However, the organization presents some difficulties. An introductory chapter contains a historical sketch of the order; the remaining chapters are organized along topical lines with little regard for chronology. It appears that for the most part the values and aims of the order have remained constant.

A chapter on membership shows that in their criteria for membership and in their programs black Masons are committed to the same values which Booker T. Washington preached—middle-class morality, cleanliness, temperance, thrift, dignity of work, and acquisition of property. They see their organization as mutual self-help for upward mobility. They have sought to create a tradition of charity among blacks, but in reality most of their

charitable activities, such as Masonic aid associations, death benefits, homes and schools for orphans, have been for Masons only. Black Masons are "acutely sensitive to what Caucasian Americans view as noble and ignoble" (p. 59), and their concern for white opinion has caused an estrangement from the black lower classes. In spite of their acceptance of white values and their striving for white approbation, black Masons were early forced to abandon hope for brotherhood with white Masons and integration with white lodges. They were always forced to organize separate lodges. Hence, they have concentrated (usually without success) upon obtaining from whites recognition of the legitimacy of Prince Hall Masonry rather than integration with white lodges.

Since the days of Prince Hall, Masonic leaders have been concerned with civil rights. They have had friendly relations with the NAACP. But Muraskin shows that Mason's values and their belief that discrimination is a class problem rather than a race problem have inhibited their effectiveness in the struggle for rights for blacks.

EMMA LOU THORNBROUGH  
Butler University

J. LEITCH WRIGHT, JR. *Britain and the American Frontier, 1783-1815*. Athens: University of Georgia Press. 1976. Pp. xii, 251. \$9.50.

For three decades following 1783, the settlement between Britain and the United States looked impermanent. In the frontier region uncertainty persisted, and Britain's equivocations exacerbated it. The British aspired to a commercial role in the Mississippi Valley and on the Gulf Coast. Their hopes for boundary rectification in the northeast and the northwest drew on the vitality of a concept that underlay the Quebec Act of 1774—namely, the economic unity of the Ohio Valley, the Great Lakes region, and the St. Lawrence Valley. Britain's shufflings toward these broad aims were affected by Indian problems and aspirations, by shadowy or shady activities of "assorted British subjects," by American politics, and by events in Europe throughout the period. The overarching question was whether the American republic could endure.

If Britain's desires are clear, specific policies for accomplishing them are not always easy to identify in this book. The reader will find it helpful to read the last chapter first. Because J. Leitch Wright, Jr. draws upon correspondence of people in and out of office, private designs appear sometimes to overlap or intersect Whitehall's. The reader has to be alert for such distinctions in this diplomatic history of the frontier written in a densely packed style, which also demands alertness.

Though it concentrates upon frontier problems, the book does not give a frontier-oriented explanation for the War of 1812. Wright merely offers casual if not wholly consistent remarks on this controversial subject. "Maritime and frontier disputes brought on" the conflict, though "maritime grievances were more serious" (p. 158). The peace treaty did not resolve "the vital issue of maritime rights that supposedly" caused the war (p. 179). The treaty, given authority by the Battle of New Orleans, nevertheless ended an era, and so the war was not useless. Britain surrendered the idea of a commercial career in the Mississippi Valley, having failed to reshape the American frontier. This book, emphasizing the unity of its frontier problems, might shade traditional perspectives of the period between 1783 and 1815 when British diplomats, official and self-appointed, tried to exploit weaknesses in the new American republic.

CARL B. CONE  
University of Kentucky

HOWARD I. KUSHNER. *Conflict on the Northwest Coast: American-Russian Rivalry in the Pacific Northwest, 1790-1867*. (Contributions in American History, number 41.) Westport, Conn.: Greenwood Press. 1975. Pp. xii, 227. \$13.95.

Howard Kushner argues that the traditional account of the Alaskan purchase errs by accentuating Russian-American friendship and the Russians' eagerness to rid themselves of relatively worthless territory. He stresses instead a pattern of mutual irritation as the Russian-American Company regularly, if ineffectively, tried to bar fur trappers, whalers, and other Americans from its domains. He also concludes that the Russians grudgingly sold Alaska only after decades of intermittent but gradually increasing American pressure.

*Conflict on the Northwest Coast* has signal merits. Kushner's bibliography is admirable, including manuscript, archival, and secondary sources in Russian and English; his lengthy and informative notes demonstrate how well he has used them. He adroitly shows how New England whalers and such concerns as the American-Russian Commercial Company and the Collins Overland Line Company, augmented by potent Congressional support, eroded Russian resistance and finally made untenable their hold in the Pacific Northwest. He writes clearly, if without much flair.

There are, however, flaws. Kushner's chapter on the Alaskan sale of 1867 is by far his weakest. His decision to tell this complicated story in merely twelve pages compels him to omit, among other things, the close first Senatorial vote on the treaty and the role of Stoeckl's bribery in terminating the

long House delay on appropriating the necessary funds. His index is annoying, and the reader seeking a specific fact about the Russian-American Company is aided little by forty-four unidentified page numbers under that heading. One almost illegible map is provided for a text studded with obscure place names. Furthermore, his thesis is hardly as revolutionary as he says. Few teachers of American diplomatic history would emphasize a theme of persistent Washinton-St. Petersburg friendship when the hostility between 1815 and 1823 is so well known, or swallow that venerable chestnut that America bought an unwanted tract to repay Russian Civil War support. Nonetheless, Kushner's scholarly work, especially on the years from 1825 to 1860, brings into sharper focus early Russian-American relations.

DAVID F. LONG  
*University of New Hampshire*

ADELE OGDEN. *The California Sea Otter Trade, 1784-1848*. (The California Library Reprint Series Edition.) Berkeley and Los Angeles: University of California Press. 1975. Pp. ix, 251. \$12.75.

In 1941, with little fanfare, the University of California Press published Adele Ogden's *California Sea Otter Trade*. This solid piece of work deserved more attention than it originally received. To write her book Adele Ogden gleaned information scattered in libraries and private holdings from California and Mexico to New England. She pieced together what was then and, in many respects, is still the best account of the search for wealth by the determined men who hunted the sea otter off the California coast during the time of Spanish and Mexican sovereignty.

Initially, the Americans and Russians collaborated in their endeavors, but later the Russians went into business by themselves. The story of the American attempts to deal with venal Spanish and Mexican officials is incompletely covered by existing records, and so this tale must at times be thin. Adele Ogden, as a scrupulously careful historian, refrains from suppositions or conjectures which might complete the account. As a result, this book is at times hard reading. Also, the author makes no concessions to the reader by explaining difficult terms. Yet the book unravels, as well as hard evidence permits, the sea-otter trade, which was an important facet of several maritime enterprises that laid the economic foundation for the American occupation of California.

The appendix includes a list of vessels engaged in the California sea-otter trade from 1786 to 1848; this is of great value to any serious scholar of early California history. Since the original book is merely reprinted, the treatment of the subject is dated.

For example, the author registered a few mild remarks condemning the sea hunters' methods and results; the searing indictment which an environmentalist of today would deliver is missing.

Yet on balance, Adele Ogden's research has stood the test of time, and it is fortunate that the University of California Press has reissued this out-of-print book.

RALPH J. ROSKE  
*University of Nevada,  
Las Vegas*

J. MEREDITH NEIL. *Toward a National Taste: America's Quest for Aesthetic Independence*. Honolulu: University Press of Hawaii. 1975. Pp. xiii, 402. \$12.00.

What were American attitudes toward the arts in the first generation after the Revolution, and when did the United States achieve esthetic independence? These are the central questions addressed by J. Meredith Neil in *Toward a National Taste*. Less concerned with artistic and literary productions themselves than with esthetic theory, Neil's major sources are articles conveying esthetic opinions published in American periodicals between 1783 and 1815. To supplement these he has also utilized the papers of such people as Charles Wilson Peale, Benjamin Latrobe, Robert Livingston, and Thomas Jefferson.

The American Revolution created a new nation. Yet political independence did not assure esthetic and cultural autonomy. Colonial life had conditioned Americans to evaluate their artistic achievements by European standards. Judged by such criteria, American painting, sculpture, music, and literature appeared second-rate. Consequently, Americans suffered from a provincial inferiority complex that persisted beyond independence. Though patriots demanded American cultural freedom, achieving it was no easy matter. In addition to needing a new esthetic and distinctly national artists, Americans had to overcome both moralistic and materialistic biases against the arts.

Neil's thesis is that this first generation of Americans was generally successful in overcoming these obstacles and creating a uniquely American artistic taste. "By 1815," he concludes, "the ground had already been well prepared for an American art." For those who have approached American cultural nationalism largely through literature, dating American cultural independence before 1815 may seem premature. After all, Cooper and Irving were not widely popular in America until the 1820s, while Emerson's "American Scholar" address, often cited as America's literary declaration of independence, was not delivered until 1837. Neil indicates, however, that, although an Amer-

ican literary standard was slow to develop, this was not the case in painting, music, and architecture and in the general appreciation of the nation's natural beauties. Most important in achieving an American taste, according to Neil was the shift from neoclassicism toward romanticism. The romantic disposition was much more supportive of already present American traditions and nationalism.

Neil's book is a useful corrective. It should add to our understanding of the cultural history of the early republic. Unfortunately, however, it reads too much like the doctoral dissertation it once was. The writing often bogs down in details and quotations, forcing the reader to search carefully for significant generalizations.

DOUGLAS T. MILLER  
*Michigan State University*

HENRY BLUMENTHAL. *American and French Culture, 1800-1900: Interchange in Art, Science, Literature, and Society*. Baton Rouge: Louisiana State University Press. 1975. Pp. xv, 554. \$17.50.

This volume is the outcome of an ambitious and wide-ranging study. The aim was nothing less than an exploration of the reciprocal reactions of the United States and France to the social and economic organization, the political institutions, the religious beliefs and practices, and, in a very general way, to the cultures of each other. "Culture," in a sense that the French often lend to the word, embraces law, philosophy, fiction and poetry, the theater, music, dance, and all the other arts. Finally, two chapters, the most original in the book, are devoted to the cooperation between the two countries in the natural and medical sciences.

Henry Blumenthal's book is not characterized by a philosophical approach or by originality. The style is clear, but not especially distinguished. Many of the chapters consist of strings of enumerated titles, and the author drops too many names indiscriminately. Every journalist or author dealing with some aspect of American civilization or of French life is granted a few lines. Such a study inevitably has to rely on much second-hand material, since the author cannot be expected to hold profound, or even personal, views on every aspect of American and French intellectual life. The few truly significant interpreters of America—Tocqueville, Chevalier in the 1830s, Laboulaye, Montégut, and later Huret—hardly receive more attention than the authors of hasty articles, probably more representative of the French middle class. Conventional topics recur over and over again: for example, the sarcasms of Stendhal, Balzac, Baudelaire, and their successors concerning the materialism, the greed, the soulless cult of tech-

nology, and the nefarious effects of America's democratic leveling.

Similarly, on the American side the same clichés monotonously recurred throughout the century: the levity and turbulence of the French, their political fickleness, the hypocrisy lurking beneath the glitter and polish of their elites, the wickedness of their papist faith, and, even more, the sinfulness of their unbelief. Auguste Comte, Balzac, Flaubert, and Zola were naturally branded as immoral. Some of the most relentless indictments of French "corruption" came from preachers and politicians, but also from Mark Twain and Henry James. The French, as is well known, lavished praise on Fenimore Cooper, Edgar Allan Poe, and Longfellow, but they failed to see any value in American thinkers (William James excepted), artists, musicians, or even scientists. The great wave of admiration for American cultural vitality came only after 1919, and chiefly after 1930.

If this painstaking, amply documented, and well-organized study reads at times like a monotonous survey and fails to open up new vistas, it nevertheless constitutes a useful reference work. It is compiled with objectivity (Louisiana, not unnaturally, receives special attention) and with wisdom. The material contained therein is very handsomely presented.

HENRI PEYRE  
*City University of New York,  
Graduate Center*

DAVID F. ALLMENDINGER, JR. *Paupers and Scholars: The Transformation of Student Life in Nineteenth-Century New England*. New York: St. Martin's Press. 1975. Pp. 160. \$12.95.

David Allmendinger revises the traditional assessment of antebellum colleges as anti-intellectual, retrogressive, committed to an archaic curriculum, and totally controlling student life. He stresses instead the dramatic changes that New England's colleges underwent in the first half of the nineteenth century. In particular, Allmendinger argues that a dramatic influx of poor students—students whose families could not fully support them at college—in search of social mobility, often through preparation for the ministry, altered modes of student support, undermined the "collegiate way of life," and revised the intellectual substance of the college experience. The bureaucratization of charity caused by this influx freed students from older, paternal forms of benevolence. The bureaucratization of the colleges was, in turn, a response to the growing autonomy and self-reliance that students demanded.

Perhaps the most striking chapters are on the



decline of the paternal collegiate community: the breakdown of the dining commons, the fragmentation of the residential college, the growth of alternative living arrangements, and especially the increasing number of students who broke the rhythm of schooling by going to work. These forced an abandonment, perhaps overstated by Allmendinger, of the old model of the college community, the *in loco parentis* system of government. In its place evolved a disciplinary system that looked to competitive grading and families to enforce order. Through a new emphasis on daily grading procedures for academic performance, the colleges sought to reinstitute control over their heterogeneous student population. Whereas grades had once been transmitted directly to students, by mid-century they were being reported to parents. The academic merit system originated not as a means of elevating scholarship but as a mechanism for disciplining students.

If at times Allmendinger's arguments are too sharply drawn, and if his use of the term "pauper" to refer simply to youth who had less money than the children of the elite that attended colonial colleges is confusing, *Paupers and Scholars* is nonetheless a significant addition to the historiography of higher education and youth in America. One hopes that Allmendinger will continue his studies into the twentieth century.

MARVIN LAZERSON  
*University of British Columbia*

RALPH ADAMS BROWN. *The Presidency of John Adams*. (American Presidency Series.) Lawrence: University Press of Kansas. 1975. Pp. x, 248. \$12.00.

This volume is part of a series that aims "to present historians and the general reading public with interesting scholarly assessments of the various presidential administrations." This purpose is generally achieved, but the work falls short of a balanced synthesis of the presidency under Adams and is limited in its scholarly contributions. The main focus of the book is on foreign affairs, particularly on Adams' success in avoiding war with France; this is seen as the central theme of his presidency. Adams is described as a dedicated leader striving for an honorable peace and willing to risk the disintegration of the Federalist party to guarantee that peace. Domestic policies do not receive equal treatment. While Ralph Adams Brown is critical of the Alien and Sedition Acts, he suggests that their significance has been overstated. The election of 1800 is not treated very thoroughly, the author arguing that Adams' attention in the summer of 1800 centered on peace negotiations, not on the political campaign.

Furthermore, he suggests that Adams was not crushed by his election defeat.

There is only sketchy treatment of Adams' conduct of the office of president. His administrative practices are described only in general terms, although Brown argues that Adams did not allow the affairs of state to suffer during his long absences from the capital. Adams' relations with his cabinet are not probed so much as one would expect in a study of his presidency. More elaboration and documentation would have been helpful in support of the view that Adams saw the presidency much as Jackson did thirty years later, viewing the president as the one agent chosen by all the people.

While indicating that Adams' reputation has risen as a result of the scholarship of the past several decades, Brown's approach still seems aimed at rehabilitating an underrated president. He emphasizes Adams' "excellence as a leader," his belief in a strong, independent executive, and his devoted service to his country. He describes Adams as purposefully delaying actions and decisions that might later restrict his freedom of action and regards him as skillful in upsetting opponents with unexpected moves.

The index is surprisingly inadequate; even the references to persons mentioned in the text are incomplete.

NOBLE E. CUNNINGHAM, JR.  
*University of Missouri,  
Columbia*

MARC FRIEDLAENDER and L. H. BUTTERFIELD, editors. *Diary of Charles Francis Adams*. Volume 5, *January 1833–October 1834*; volume 6, *November 1834–June 1836*. (The Adams Papers, First Series.) Cambridge: Belknap Press of Harvard University Press. 1974. Pp. xlii, 413; xviii, 469. \$40.00.

Volumes 5 and 6 incorporate Charles Francis Adams' diary from January 1833 through June 1836, advancing the author from the age of twenty-five to almost twenty-nine years. In this time frame, the death of his brother and the birth of his own two sons cause him to compare as well as to contrast the Adamses' progress with the saga of the nation's other first families. As the diarist was wont to do, he saw Divine Providence favoring the grandfather, John Adams, when reviewing the fates of lesser presidential persons after they relinquished the highest office in the land.

Washington, he recorded with seeming satisfaction, had no children and twenty years less of life. As to the others, "Jefferson died a bankrupt with bitter private griefs and nothing to compensate for them." Madison, he noted, was childless; and



Monroe, after long years of pecuniary distress and mortification, died a bankrupt. But highly significant to the Adams family's sole representative in this generation was that "all of them have had their sceptres wrenched by an unlineal hand, no son of theirs succeeding."

Although engaged in historical activities (sorting the family papers and undertaking in behalf of his father political writing which he signed with pseudonyms), he nonetheless railed against the way other historians and political journalists were treating the Adams family. As he recorded on March 5, 1833, history was such that even posthumous fame depended upon the miserable conceits of this or that prejudiced brain. Throughout the diary he laments that history should make Jefferson into an idol and cause his grandfather to be detested, that Jackson should be lauded to the skies and his own father persecuted. So much, the diary seems to be saying, for the view that although the Adamses and the Jeffersons were estranged in the 1790s, there was a reunion of the families following the War of 1812.

To Charles Francis Adams, the grandfather, rather than the father, was perhaps the greater man. The reason given is that "[John Adams'] private letters display tastes equally strong for the enjoyment of private life," whereas John Quincy Adams "still is agitated by the restless worm" (which the editors define as "political ambition"). Yet the grandson himself is agitated by the same "restless worm." Why else would he publish and enjoy satisfaction from his own authorship of political tracts appearing in the Boston *Daily Advocate*? On October 3, 1835, he perhaps explained his move into unaccustomed political activity by writing that "I have the misfortune of being a descendant of two great men, and must do something to avoid the charge of utter degeneracy."

Adams' crankiness, as well as his stuffiness, is revealed in his reflections on college days, his estimates of contemporary politicians, and his criticisms of social events. As to holidays, one wonders what Adams would have thought of the nation's bicentennial celebration. As noise on the occasions of Independence Day was not to Charles Francis Adams "a necessary concomitant of rejoicing," he felt duly grateful for the enjoyments of the day "which were to be found in profound quiet."

The editors have maintained the same high standards of the earlier publications. They have provided a descriptive list of illustrations, an introduction that considers the 1833-36 time period as well as the manuscripts and the editorial method, a guide to the editorial apparatus, a chronology of major events in the diary, and a detailed index. Best of all, they have permitted the

reader a glimpse of the agonies and the ecstasies of being an Adams, a difficult assignment in the days of Jacksonian Democracy.

WILLIAM T. DOHERTY  
West Virginia University

STEPHEN E. BERK. *Calvinism versus Democracy: Timothy Dwight and the Origins of American Evangelical Orthodoxy*. Hamden, Conn.: Archon Books, 1974. Pp. xiv, 252. \$14.50.

Early in the nineteenth century, New England Calvinism abandoned the pastor's arid study for the arena of public needs and aspiration. Stephen E. Berk's book intends to reconstruct Timothy Dwight's contribution to this transformation in American theology. Dwight deserves renewed attention; as grandson of Jonathan Edwards, president of Yale, man of letters, and Connecticut divine, his influence was considerable until his death in 1817. Thereafter, several of his numerous intellectual progeny, such as Nathaniel W. Taylor, Lyman Beecher, Charles G. Finney, and Horace Bushnell, took further steps in unifying Calvinism with humanitarianism. The changes in Dwight's theology and his personal perception of the tumult around him would challenge any author. However, Berk often puts Dwight aside to attend to such larger issues as nationalism, economic upheaval, democratic insurgency, and other matters implied by the ponderous divisions of his volume—Religion and Social Control, Theology and Change, and the New Orthodoxy. Unfortunately, space and preparation are lacking for an important treatment of these topics, and Dwight goes without a careful and extended analysis.

Certainly there are tempting glimpses of Dwight as he delicately maneuvered around Consistent Calvinism with its insistence upon absolute sovereignty and the insignificance of man's action. This viewpoint gave way to a New School orthodoxy, allowing Dwight gradually to accept man's voluntary power, his capacity to benefit from a wholesome environment, and his zeal for a materially productive life. Berk presents Dwight as an "inveterate" utilitarian who was a "transitional fusion of Edwards and Franklin" (pp. 111, 192). Although this picture begs for enlargement, Berk does offer brief assessments of some important figures in American religious history. In addition to Taylor and Beecher, these include Joseph Bellamy, Jonathan Edwards the younger, Nathanael Emmons, Samuel Hopkins, and Jedidiah Morse.

This volume is characterized by the author's persistent unhappiness with the values of nineteenth- and twentieth-century Protestantism. Berk indicts Dwight and his followers as the unwitting

abettors of ethnocentrism and the cult of material progress, while announcing his own pleasure at the rise today of social evangelicalism.

PAUL C. NAGEL  
University of Missouri,  
Columbia

LESTER G. MCALLISTER and WILLIAM E. TUCKER. *Journey in Faith: A History of the Christian Church (Disciples of Christ)*. Saint Louis: The Bethany Press. 1975. Pp. 505. \$12.95.

This book is a revision of the standard survey, *The Disciples of Christ: A History*, first published by Winfred Garrison and Alfred DeGroot in 1948. A generation of scholarship has altered perspectives and added substantially to the historical literature on the Disciples since that earlier date. Revision has eliminated the pronounced emphasis on frontier and sectionalism, obvious Turnerian influences on Garrison and DeGroot. So, too, have McAllister and Tucker avoided the arbitrary chronological and thematic division of materials that dominates the last half of their predecessors' denominational history. The result is a smoother, better-integrated survey that successfully combines "chronological and thematic approaches" to the history of a church "in process of development" (p. 10). The greatest emphasis is upon institutional rather than ideological or theological development, although the latter certainly is not ignored.

The authors assert that a central concern was not to view "the Stone-Campbell movement in isolation," but instead "within the context of Christianity in North America" (p. 10). However, their commendable attempts in this regard could have been expanded even further. The reader fails to sense, for example, the full representativeness of the Disciples as a major component of nineteenth-century evangelical Protestantism. McAllister and Tucker place considerable emphasis upon the Disciples' use of a popular periodical press as a means to advance their cause throughout the nineteenth century. This was not a unique concern, but a widespread practice among all evangelical groups, a fact the authors fail to demonstrate properly. And in their discussion of the Disciples' creation of a bible society in the 1840s, of the use of institutionalized revivalism and itinerant ministry, of the growing interest in overseas missions in the late nineteenth century, the authors could have more fully established connections with the larger American Protestant community.

The demand that this be a comprehensive survey of the denomination's history sometimes engulfs the writers as analysis declines into simple narrative and occasionally into a bewildering catalog of boards, schools, and denominational leaders. The last chapters on recent Disciples history

are often plagued in this regard. In essence, McAllister and Tucker are describing the ecclesiastical phase of the vast bureaucratization that has affected all of American society since at least World War I, but such an interpretive framework is overlooked.

Although they have missed important opportunities to reveal the full historical meaning of their work, the authors have produced an effective summing up of contemporary scholarly understanding of a major American denomination that for too long has remained in the shadow of other longer-established, better-known Protestant groups.

JAMES FINDLAY  
University of Rhode Island

JOSEPH E. WALKER, editor. *Pleasure and Business in Western Pennsylvania: The Journal of Joshua Gilpin, 1809*. Harrisburg: Pennsylvania Historical and Museum Commission. 1975. Pp. vii, 156. \$6.00.

Taking the evening plane to New York, catching the 9:10 to Washington, or hopping in the family car to drive to the shore for a weekend is an absolute necessity of the contemporary American lifestyle. The possibility of another Arab oil embargo that might drastically affect this freedom of movement produces trauma across the land. In such a milieu the journal of a Philadelphia entrepreneur's travels through western Pennsylvania to Pittsburgh and back again in 1809 might seem prosaic and quaintly irrelevant.

In fact it is an impressive contribution to an important genre of historical scholarship, the edited travel journal. Joshua Gilpin was, in the language of his day, a man of means. Gilpin, from an established Philadelphia family, had substantial holdings and considerable future interest in the new country west of the mountains. In 1809 he, his wife, eight-year-old son, and several servants set out by carriage to travel west to Pittsburgh to examine both the country and its investment prospects. Over a month later, after thousands of bumps, a potpourri of good inns and bad, a glimpse of how the other half lived, and hundreds of observations about what they had seen, the Gilpins returned to the comforts of Philadelphia to recuperate and reflect.

Gilpin's 152-page journal is crammed with notes covering such diverse topics as terrain, soil quality, flora and fauna, land values, commodities in trade, crops, social conditions, local religious and ethnic groups, and the temperament of local innkeepers.

Gilpin had a good education, a keen mind, the eye of the natural diarist, and the instincts of a successful promoter; little eluded him, and we are the fortunate beneficiaries. Especially interesting are Gilpin's concern that Baltimore, Phila-

delphia's commercial rival, might win the race to tap the potential of western Pennsylvania and his comments on the persistent sensitivity of westerners to the Whisky Rebellion of fifteen years earlier.

Joseph E. Walker has obeyed the two most important canons of the diary-journal editor—to anticipate all of the reader's questions and needs for assistance while maintaining as low a profile as possible. Walker's editing is unobtrusive, scholarly, and extremely helpful.

JOSEPH W. COX  
Towson State University

ERIK F. HAITES, JAMES MAK, and GARY M. WALTON. *Western River Transportation: The Era of Early Internal Development, 1810-1860*. (Johns Hopkins University Studies in Historical and Political Science, 93rd Series, 1975.) Baltimore: Johns Hopkins University Press. 1975. Pp. xi, 209. \$12.50.

Before the nineteenth century the United States was primarily an agricultural country. The first half of the 1800s resulted in a great increase in manufacturing. This development could not have taken place without the availability of a good transportation system provided chiefly by the rivers and canals. Railroads were being built, but water transportation was more important.

Much has been written on the development of manufacturing during the pre-Civil War days. The building and use of canals has received much attention, but river transportation has been neglected until the publication of this book. The rivers were used by steamboats and flatboats.

The federal government contributed to the development of river transportation. Topographical engineers made several studies, with expeditions in 1817, 1818, and 1823, under the command of Major Stephen Long. General Bernard and Colonel Totton made reports on the navigation problems on the Ohio and Mississippi rivers. The falls were the major obstacle to navigation on the Ohio River. The river had a narrow channel with shoals of sand and gravel extending across it in several places. The chief difficulties on the Mississippi River were snags resulting from trees. Another problem on the Mississippi was the shifting current.

The river improvement activities of the federal government focused on two objectives. The first efforts concentrated on removing natural obstructions such as falls and rapids. Second, the government removed snags and sunken boats that were of recent origin. The government concentrated its efforts on the falls of the Ohio (Louisville), the Muscle Shoals, and the rapids on the upper Mississippi.

Since the presidents beginning with Andrew Jackson took a strict constructionist view of the Constitution, appropriations by the federal government were rather limited. Of all the antebellum presidents, Pierce was most opposed to appropriations for river improvement.

This book is recommended reading for all interested in river transportation.

JOHN H. KRENKEL  
Tempe, Arizona

EUGENE ALVAREZ. *Travel on Southern Antebellum Railroads, 1828-1860*. University: University of Alabama Press. 1975. Pp. x, 221. \$8.95.

KEITH L. BRYANT, JR. *History of the Atchison, Topeka and Santa Fe Railway*. (Railroads of America Series, volume 3.) New York: Macmillan Publishing Company. 1975. Pp. xvi, 398. \$12.95.

Keith L. Bryant, Jr.'s *History of the Atchison, Topeka, and Santa Fe Railway* is the most comprehensive account published since 1950. Written in a form and style that should satisfy both the scholar and the railroad buff, the book details financing, construction, growth, and management from the Santa Fe's conception by Cyrus K. Holliday in 1859, to its evolution into a giant conglomerate in the 1960s. After construction began in 1868, company leaders shrewdly built the railroad toward known resources—coal southwest of Topeka, cattle in south central Kansas, and the greatest prize, a federal land grant when the railroad reached Colorado at the end of 1872. Thereafter, wider horizons beckoned toward the Pacific.

Bryant has organized his study chronologically around the administrations of successive Santa Fe presidents. At the same time, he pursues such topics as colonization efforts, the development of steam and diesel locomotives, the vicissitudes suffered by the railroad during wars and depressions, the effects of technological change, and, after 1945, the Santa Fe's surprising and expensive efforts to attract passengers in the face of growing competition from automobiles and airplanes.

The Santa Fe Railway is hardly a neglected topic, as Bryant's references to numerous books, articles, and dissertations on various aspects of the railroad's history make clear. Some of what the author includes (for example, Holliday's vision and praise for Fred Harvey's excellent food) is quite familiar. The company made available a "monumental body of primary sources" so "immense" that "no single historian, or even a group of scholars, could ever hope to see" everything. Footnote citations and the author's bibliographical essay indicate that he relied heavily upon annual reports and the "Santa Fe Splinters," a body of records compiled for the company in 1940 by

Joseph Weidel. The minutes of the board of directors, Bryant states, were "the only significant corporate records" withheld. This being the case, some may question the almost total neglect of company correspondence.

Generously adorned with photographic illustrations, mostly from the Santa Fe archives, the book also provides seventeen maps depicting various stages of construction and growth. Useful as these maps are, some readers will probably wish to consult others that are more complete in order to fully comprehend the railroad's development. In regard to construction, I searched modern gazetteers in vain for the Kaw River (pp. 8, 15). But this is a minor problem. Bryant's book is well organized and written and interesting reading, with thoughtful and balanced judgments.

Eugene Alvarez has written a social history around antebellum Southern railroad travel. He did his research in several contemporary periodicals and over a hundred travel books written mostly by Europeans during this age of the grand American tour. Some of the railroad material will sound familiar to the specialist, but in bringing together a variety of scattered travel accounts, Alvarez has performed a valuable service. The result is a well-written history that should please the general reader as well as the scholar. It is unfortunate that the book's few illustrations are neither authenticated nor listed after the table of contents.

If *Travel on Southern Antebellum Railroads* contains a thesis, it is that such travel promoted a "democratic atmosphere." The design of the American passenger car, with its center aisle, open seating, and unreserved sleeping berths, startled class-conscious Europeans who were not prepared for the "simplicity and equalitarianism found on American trains." Even railroad stations had a "peculiarly equalitarian flavor." Few will quarrel with these assertions. The author could have made his case even stronger, however, if in the final chapter he had confined himself to a summary and analysis of the democratic theme and the evidence, instead of launching briefly into a discussion of Southern railroads during the Civil War. Neither the book's title nor its content justified such an excursion.

MERLE E. REED  
Georgia State University

DAVID E. SCHOB. *Hired Hands and Plowboys: Farm Labor in the Midwest, 1815-60*. Urbana: University of Illinois Press. 1975. Pp. viii, 329. \$10.95.

Initially, one is tempted to dismiss this book as an academic rendition of the survival farming tracts of the *Foxfire* genre, but that would be an injustice. Actually, David E. Schob is to be commended for

his attempt to relate the story of farm labor in the Midwest from 1815 to 1860. As Jesse Lemisch and Staughton Lynd have noted, historians know little about working-class life in early America, and we have not seriously begun to fill the void—to engage ourselves in the arduous task of writing "history from the bottom up."

Schob examines the work of clearing land, breaking prairie, teamstering, and harvesting, as well as the digging of drainage ditches, wells, and cellars. He also explores the special application of labor in horticulture, winter-seasonal employment, and the roles of hired girls and boys. In two chapters he considers the treatment of hired hands, their terms of employment, and their leisure time and recreational opportunities. Schob treats these subjects in minute detail, and his discussion is based on extensive research in manuscript collections, effectively supplemented by printed primary and secondary materials. Certainly, the book is a storehouse of information on the functions of early nineteenth-century midwestern farm labor, and it is based on meticulous research.

But should it be more than that? Schob notes that the Midwest is "ideal" for a study of "labor and its adaptation to [the] various stages of agricultural development" (p. 2). Yet the study somehow fails to realize this potential. Perhaps the limitation of sources is the problem; Schob concedes that the extant evidence is not "abundant." More likely it is the topical organization and the chronological time frame utilized. Indeed, farmers "did not mechanize their operations until during and after the Civil War" (p. 109). Thus this critical development in farm labor's adaptation lies beyond the scope of this work. In addition, the focus is on what farm laborers did and how they did it, not on how they felt and lived. More critically, the study is not related persuasively to the broader context of American history, and the misplaced attempt at comparative analysis in the concluding chapter is a case of too little, too late. Nonetheless, *Hired Hands and Plowboys* is a useful piece of spadework.

GENE CLANTON  
Washington State University

ERNEST R. MAY. *The Making of the Monroe Doctrine*. Cambridge: Belknap Press of Harvard University Press. 1975. Pp. xviii, 306. \$12.50.

After all the effort that has been spent on the study of the Monroe Doctrine over the past two generations, the appearance of another book on the subject immediately raises the question why. What is left to be uncovered? Ernest R. May believes that a new angle of observation would yield



new insights into the behavior of the major actors on the scene—Adams and Monroe, Calhoun and Clay. But even as he places his name on the long roster of authorities on the Monroe Doctrine, he does so gingerly, listing the leading authorities from Bemis to Whitaker by alphabetical order and successfully avoiding any hint of rank.

The author makes good his justification for a new book. It is not simply that he has done a thorough job of research, ranging beyond the scope of his predecessors, or that he presents his findings in a prose that is a pleasure to read. He approaches the subject by asking himself what issues would have impressed well-informed contemporaries as determining policy decisions in 1823. May raises and discards the personal convictions of the policymakers as vital factors. The differences in substance were minimal. He also rejects the hypothesis that the Monroe Doctrine was the product of international politics that left few choices to American diplomats. He judges that they had made an accurate assessment of the situation abroad in 1823. Trouble with Russia in North America, intervention by France in Latin America, or rapprochement between Britain and the Continent were all unlikely prospects, and the American policymakers knew it.

What counts in May's scenario is the dominance of domestic politics over the behavior of the major political figures. Most of them—Clay, Calhoun, Crawford, Adams—were anticipating the presidential campaign of 1824. Adams was as ambitious as any of his competitors, and he did his best to reconcile conscience with political interest. His most memorable stance, opposing collaboration with Britain, is seen as the effort of a former Federalist to disarm his critics rather than as an expression of a fear of entanglement or even of too junior a partnership. Adams "could have reasoned," May notes, "just as easily as Jefferson that a concert with England would guarantee America's independence, security, and peace" (p. 255).

May never pretends that he has read Adams' mind. The text is filled with caveats to remind the reader that this is hypothesis. When he suggests that Adams recognized "at least subconsciously that if it were Madison who signed the accord with England, critics would find it much harder to call it the work of a crypto-Federalist secretary of state" (p. 205), he includes a "probably" in his judgment. The result is an interesting and convincing approach to the Monroe Doctrine as the president's message is set in a context of competition among able presidential candidates using foreign politics in the absence of domestic issue to win nomination. The physical appearance of the book is as attractive as its contents, although the 260 pages might have been severely reduced with-

out affecting the thesis. The surveys of politics in American and Europe in the first half are superfluous, but they are worth reading as a colorful preface to an imaginative work.

LAWRENCE S. KAPLAN  
Kent State University

W. EDWIN HEMPHILL, editor. *The Papers of John C. Calhoun*. Volume 8, 1823-1824. Columbia: University of South Carolina Press, for South Carolina Department of Archives and History and South Caroliniana Society. 1975. Pp. xlviii, 674. \$25.00.

Volume 8 of the *Papers of John C. Calhoun* is likely to be one of the most important in the entire series, not because of what it reveals about Calhoun, but because of its wealth of information and insight on the presidential campaign of 1824. This volume is edited in the same unique personal style which characterized previous volumes in the series. It includes transcripts or abstracts of all incoming and outgoing documents "no matter how minor their contents may appear to be," avoids separate notes in favor of interlinear emendations, and identifies all persons no matter how significant each time they are mentioned. The volume is remarkable for the amount of relatively new and untapped information it contains.

Covering a full year beginning in April 1823, the documents roughly correspond with Calhoun's forty-second year and his seventh as secretary of war in the Monroe administration. Since it charts in detail his pursuit of the presidency and his nomination for the vice-presidency, the volume reveals one of the crucial turning points in Calhoun's career as well as one of the great moments in American electoral politics. Because the United States had not yet invented a workable system for selecting its presidents and since the campaign of 1824 saw the failure and demise of the caucus system, the volume is a welcome reference work for a period which has not received the careful scrutiny of historians. Although the volume contains a good deal of information on the concerns of the secretary of war, from Indian affairs to the United States Military Academy and from the Army's role in internal improvements to Calhoun's role in the development of the Monroe Doctrine, much of this material is deadeningly routine and uninspiring, and will not be of any substantial use for social, intellectual, or diplomatic historians.

Historians will be pleased with Calhoun's observations on the five-cornered struggle between himself, William H. Crawford, John Quincy Adams, Henry Clay, and Andrew Jackson. In the absence of a party system, Calhoun and his rivals attempted to create coteries of supporters and to



enlist the press in each state as a means of winning official endorsement from state legislatures. In spite of his eventual identification with the South, Calhoun is revealed as having notably strong followings in various northern states. Readers with an interest in Calhoun the man will be disappointed that the documents do not show him as the thoughtful, intelligent figure he is often interpreted as having been. Nor will readers be particularly convinced by the editorial perspective which measures all things in relationship to the rather limited role Calhoun is revealed to have played, despite his office, in American affairs in 1823.

LARRY E. TISE  
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Archives and History

FRANK FURLONG MATHIAS, editor. *Incidents & Experiences in the Life of Thomas W. Parsons from 1826 to 1900*. Lexington: University Press of Kentucky, 1975. Pp. xix, 209. \$13.50.

Frank Furlong Mathias has done superb work in editing the diary of Thomas W. Parsons. This diary includes new information about events that occurred during the last three-quarters of the nineteenth century in Mid-America. Parsons' descriptions are those of a middle-class person who lived in an expanding society.

Parson's early days in Southwestern Virginia were filled with experiences, both sad and joyful, which molded his life into a conservative pattern, and he developed a desire to deal fairly with people. When he moved to Kentucky, he found the same basic concepts of life to which he had been accustomed. His neighbors earned a livelihood by the "sweat of their brows." Perhaps the editor exaggerates the conservative qualities of Parson's life. He wanted Kentucky to remain unspoiled by political graft and economic upheavals. Although this dream was impossible, the idea never left his mind.

Parson's patriotism was characteristic of middle-class America throughout the nation's history. He joined willingly in the struggle against Mexico and endured the hardships of army life without complaint. After the war he became a schoolmaster and taught in both Kentucky and Indiana without much enthusiasm for the profession, but he was as loyal to teaching as he was to other vocations that he followed.

Parson was sympathetic to the North during the Civil War. He regretted the invasion of Kentucky by Confederate and Union armies and detested the deeds committed by guerrilla raiders who looted and killed in the name of the Confederacy or of the Union. He rode proudly with the Union cavalry to

protect Kentucky's neutrality and to preserve the nation's unity.

After the Civil War Parsons spent several decades as a bricklayer. In his own words he used his trowel, "never to lay it down for any other employment except at short intervals till I grew too old and enfeebled to wield it." The reviewer recommends the diary of Thomas W. Parsons to readers who wish to learn more about Mid-America during the nineteenth century.

RHEA A. TAYLOR  
University of Kentucky

MATTHEW A. CRENSON. *The Federal Machine: Beginnings of Bureaucracy in Jacksonian America*. Baltimore: Johns Hopkins University Press. 1975. Pp. xii, 186. \$10.00.

The paradoxical character of the Jacksonian era is nowhere better demonstrated than in Matthew Crenson's *Federal Machine*, a brief but significant study of the origins of Jacksonian bureaucratic administration. Leonard White, in his pioneering works, always emphasized the continuing links between the Jacksonian executive system and its Federalist antecedents. Crenson, however, insists that as a result of the Jacksonian impulse, there were indeed not only some substantial changes in the "spirit," but also in the "form" of the federal establishment.

Siding with such historians as John William Ward and Marvin Meyers, who have analyzed the emotional and psychological influences of Jacksonianism, Crenson maintains that the democratic demand for frequent "rotation in office" actually produced a far greater degree of bureaucratic depersonalization than previously realized.

Andrew Jackson set out to purify the federal government, to restore it to its old simplicity, and to bring back the type of personal organization originally espoused by the Federalists. The trouble was, however, that in seeking to develop this simple and personal administrative structure, the Jacksonians found that the common man was much too unreliable to be trusted on his own. Because of the deterioration of such traditional social institutions as the legal system and the business community, by the 1830s the old-fashioned virtues of honesty, integrity, and diligence were no longer operative.

In an attempt, then, to ensure honest administration within a highly personal organization, Jacksonian administrators such as Amos Kendall, in his capacity as postmaster general, developed separation of powers, clear description of duties, bureaucratic checks and balances, close supervision of all employees, and regular audits by other government agencies. Ironically, therefore, the

Democratic party, which started out pledging to return the federal government to its original simplicity, ended up by creating an elaborate, complicated, and impersonal bureaucracy.

By viewing administrative history not merely in terms of the structural components that determined its form, but also in terms of the social and political forces that shaped its spirit, Crenson has offered a fascinating and instructive model for similar historical studies.

THOMAS H. O'CONNOR  
*Boston College*

ELLEN M. WHITNEY, compiled and edited by. *The Black Hawk War, 1831-1832. Volume 2, Letters and Papers. Part 1, April 30, 1831-June 23, 1832; part 2, June 24, 1832-October 14, 1834.* (Collections of the Illinois State Historical Library.) Springfield: Illinois State Historical Library. 1975. Pp. xxi, 660; xiii, 661-1358.

Can one find happiness reviewing a volume that has already won the 1975 Award of Merit of the American Association for State and Local History?

The first volume of this series, published in 1970, contained Black Hawk War muster rolls and a historical introduction by Anthony F. C. Wallace. The second volume, in two parts, contains the letters, documents, recollections, and a few diaries and reminiscences "essential for a thorough understanding" of that war. Material has been drawn mainly from collections of the Illinois State Historical Library, and secondarily from other sources, public and private. About fourteen hundred items are published here, all in as close to "true copy" form as scholarship could wish, and all edited with copious critical commentary. Indeed, it is common to find the footnote tail wagging the document dog. Such detailed annotation and the actual publication of this source material are, according to the editor, Ellen M. Whitney, to "enable . . . scholars to undertake studies of the Illinois frontier and the dispossession of the Sauk and Fox Indians." It is certain that the scholar will not need to grub for biographical, geographical, bibliographical, or any other details, all of which are abundantly supplied. Instead, he may concentrate on interpretation.

The effort expended on assembling, collating, editing, and publishing this source material is almost appalling. The volumes are an example of what editing can become when unhampered by time or money limitations. One can ask if it is really useful to publish in footnotes biographical information readily available in the *Dictionary of American Biography*. Would it not serve as well to provide only the new or difficult-to-obtain facts?

Is any discernible purpose served by adding to the biographical note on Joseph Montfort Street that his wife's brother had a farm near Jacksonville (p. 10)? Is it possible to edit to the point of irrelevance, or is it worthwhile to publish any piece of information simply because it has been found? On the other hand, the inclusion of the whole grab bag of facts makes these volumes useful for purposes other than the history of the Black Hawk War, such as regional history and genealogy.

Historians have been predicting that publications of this nature and scope would disappear, except for editions of the papers of great men. Instead of printed and edited books, we would need to rely on microforms of one kind or another, more readily prepared and more economically issued. The historians are probably right. These volumes, and the scholarship supporting them, may represent an endangered species. Considering the important role played by this species in the past, and the value it has in this instance, it is worth hoping that the remaining specimens will be treated with respectful care and nourished, so that the breed will not become extinct.

MARTIN SCHMITT  
*University of Oregon Library*

GEORGE M. DENNISON. *The Dorr War: Republicanism on Trial, 1831-1861.* Lexington: University Press of Kentucky. 1976. Pp. xiv, 250. \$17.50.

This latest account of Rhode Island's political turmoil in the early 1840s concentrates on two themes: the relation of the controversy to American legal history and political theory, and its centrality in the transformation of American society. "Traditionalism, translated into a conservative institutionalism, displaced liberal or radical politics." The vagueness of this second theme and the imprecision of its terminology and development detract from the book's valuable discussion of the constitutional aspects of the conflict.

As a description of the events of the suffragist struggle, George M. Dennison's account is less coherent than either Arthur May Mowry's able history or Marvin Gettelman's richly researched version. Dennison presents the suffragists' case with little questioning of their evidence and little respect for the reasoning of their opponents. Often it is unclear where Dennison's paraphrase of the Dorr position stops and his own presentation begins: repeatedly the author describes the conflict, as did his subjects, as one of right versus might. Dorr's miscalculations are largely ignored in favor of praise for the leader who, we are told, had a mind like that of Emerson, "if less intellectually isolated." Even Dorr's shifting and confused defense after his movement's collapse is celebrated as

"the most brilliant [constitutional] exposition by an American theorist since James Wilson."

Dennison glorifies Dorr because he sees in the Rhode Islander an embodiment of "the Revolutionary heritage of all Americans," a tradition of popular sovereignty which stressed that the majority was to rule without any legal or procedural restraint. This position is based on no historical evidence but a definitional-legal abstraction: sovereignty is indivisible—if the people do not possess it without restriction, the government wholly engrosses it to become "the Leviathan state." What this view neglects is the fact that popular rule in the United States was always structured not as the antithesis but as an adjunct of established procedures. If structure came to be seen as inhibiting majoritarianism, as happened in Rhode Island, appeal could be made to the divested sovereignty of the people to gain support for changes in accord with public opinion. The situation in Rhode Island became perilous for two reasons. Dorr undercut this kind of "peacable revolution" by his farcical military ventures, and he refused to acquiesce when it became clear that his tactics—and his opponents' concessions—had lost him majority support. After 1842 Dorr was the institutionalist insisting on the validity of *his* constitution regardless of majority sentiment.

Around his central abstraction, Dennison sprinkles others. "No establishment existed" in the Revolutionary period. After the 1840s Americans' expectation of "unending and inevitable progress" led them to substitute "administrative rationalism" for interest in politics or democracy. And in the Civil War, "with the Rhode Island incident providing the precedents for action," the North crushed the idea of popular sovereignty with the South. For all these claims Dennison offers little evidence beyond reference to his own articles and papers. This is peculiar historical methodology: previous assertion is proof of present assertion.

The book's inflation of argument is unfortunate because Dennison has valuable points to make about American legal history. His discussion of *Luther v. Borden* makes clear the extent to which judicial hegemony undergirds the "judicial restraint" of that decision. And his handling of the question of martial law is able, though here the big idea muddies the clarity of discussion in his earlier article on the subject. With ideas, as with other things, bigger is not necessarily better.

DAVID A. GRIMSTED  
University of Maryland,  
College Park

WILLIAM STANTON. *The Great United States Exploring Expedition of 1838-1842*. Berkeley and Los Angeles:

University of California Press. 1975. Pp. x, 433. \$14.95.

Why should the United States Navy's peregrinations about the Pacific from 1838 to 1842 deserve additional book-length treatment when David B. Tyler's *Wilkes Expedition*, Philip I. Mitterling's *Americans in Antarctica to 1840*, and a succinct chapter in Vincent Ponko, Jr.'s *Ships, Seas, and Scientists* have appeared during the last fifteen years? William Stanton provides the answer in this exemplary monograph.

A single negative comment is in order. Although there are admirable black and white contemporary sketches scattered throughout the text, the lack of detailed maps is unforgivable. Two end-paper maps are provided to accompany a description of a four-year voyage through some of the lesser-known areas of the earth. If that of Antarctica is useful, the global chart, on a scale of roughly 2,000 miles to the inch, is worthless for the reader trying to follow Wilkes through the Fiji or Tuamotu Archipelagos.

Stanton's work is otherwise solid. He tells of the beginning of an expedition so riddled with controversy among the politicians, naval officers, and scientists concerned that by a sort of osmosis its command filtered down to Charles Wilkes, a mere lieutenant who would wait for promotion to commander until 1843 and to captain until 1855. That testy individual of later *Trent* notoriety was not only an inept seaman, but one whose pathological suspicion of his fellow-officers brought an eventual rash of charges, countercharges, and courts-martial. But the energetic and seemingly sleepless Wilkes somehow managed to bully his six-ship flotilla all the way from Norfolk to Antarctica, past scores of Pacific islands to Australia, back to Antarctica, through perilous Fijian waters to Hawaii, to the Pacific Northwest, and finally to New York via the Cape of Good Hope.

Stanton writes engagingly, providing particularly memorable accounts of the sloop *Peacock's* miraculous escape from Antarctica's "ice mountain," her destruction in the mouth of the Columbia River, and the almost ludicrously harrowing conditions aboard the schooner *Flying Fish*. He draws effectively upon ample contemporary sources, recounting the alarm of the expedition's taxidermist in Hawaii when the flagship had become infested with "Yellow Hores" and other "Ladies of Pleasure." He furnishes his own deft observations: Wilkes in his dotage "wrote on and on with dour gusto, savoring anew the old animosities, reappraising his career and finding . . . every decision judicious and timely. It was an exercise in introspective hagiolatry."

The author's scientific acumen stresses that the

almost twenty voluminous works published by the expedition's scientists over three decades after 1842 had lasting consequence. If, prior to 1838, the typical scientist in the United States had been a gentleman dilettante, Stanton convincingly demonstrates that Wilkes, by putting "science into government and government into science," had "made it possible for the American scientist to live by his profession."

DAVID F. LONG  
*University of New Hampshire*

GEORGE CATLIN. *Letters and Notes on the North American Indians*. Edited and with an introduction by MICHAEL MACDONALD MOONEY. New York: Clarkson N. Potter. 1976. Pp. xv, 366. \$15.00.

Interest in George Catlin, both as a painter and as an ethnographic observer, continues to rise rather than diminish. The present book is an example of that growing interest. Catlin literally gave his life to the task of recording in paint the features and activities of the natural inhabitants of the New World. His travels among the Indian tribes of the trans-Mississippi West in the 1830s are best known, but he also painted his way from Alaska to South America until his death in 1873. Like other close observers of the Indians, Catlin was an admirer of their virtue and an advocate of their cause. Nevertheless, his was a frustrating life marked by desperate efforts to gain financial support by exhibiting his paintings and writing about his travels. No one understood as well as he the value of the work he was doing. Repeated disappointments weighed upon him, however, until bankruptcy and death relieved him.

The present edition is designed to make Catlin's description of the Indians of the American West better known to a nonscholarly audience. It contains 160 black-and-white illustrations and 8 color plates. The editor, Michael M. Mooney, has supplied an 83-page introduction without footnotes that conveys, in telegraphic style, the basic outline of Catlin's life and the history of his times. *Letters and Notes*, which Catlin published in London in two volumes in 1841, is condensed and reorganized in geographical order, rather than in their original chronological order. The editor cheerfully concedes that he has "taken liberties that might cause some scholars to faint." He notes that "I have rearranged sentences, modified verbs, changed their tenses, repunctuated when I wished, omitted ellipses when they should have been included, junked all the rules about footnoting according to custom, or annotating. . . . I take responsibility for all, because I wanted to get Catlin's advocacy to the general reader once again."

The original edition of 1841 or the Dover Pub-

lication's 1973 reprint of the 1844 London edition with an introduction by Majorie Halpin (taken from her booklet, *Catlin's Indian Gallery: The George Catlin Paintings in the United States National Museum*, published by the Smithsonian Institution [1965]) will remain the appropriate editions for scholars. Mooney's edition will, in the meantime, spread a little further the fame of this still too little appreciated man.

WILL COMB E. WASHBURN  
*Smithsonian Institution*

JAMES H. SHIDELER, editor. *Agriculture in the Development of the Far West*. Washington, D.C.: Agricultural History Society. 1975. Pp. ix, 316.

This collection is the harvest of a symposium held in June 1974 and jointly sponsored by the University of California at Davis, the Agricultural History Society, and the United States Department of Agriculture. Despite the designation "Far West" in the title, the bulk of the pieces deal with California. Most are by historians, but some are by economists, geographers, or anthropologists; there is at least one bona fide dirt farmer in the lot. All are grouped under major headings to reflect the relationship of western agriculture with a number of larger questions—values in society, science and technology, law and government policy, library and archival usage, immigration, the environment, and international food markets. All of these are washed down by Carlo Cipolla's address, "European Connoisseurs and California Wines."

The volume includes much wheat and some chaff. In one of the more provocative papers, Leonard Arrington and Dean May demonstrate that the success of nineteenth-century Mormon irrigated farming in Utah was closely related to church practices and beliefs; hence, much of this accomplishment was not transferable to agriculture elsewhere. Reynold M. Wik effectively debunks the myth of the American frontier farmer as an inventive genius, applying Walter P. Webb's theory to California's Central Valley to argue that it was the adaptation of machinery to the environment that was important, but not by sodbusters on the cutting edge of population advance.

Also outstanding is Paul Gates' paper in which he traces the present-day pattern of large land holdings in California back to earlier land policies and their administration. The Gates tradition of rigorous scholarship is also apparent in contributions by several of his students: Harry N. Scheiber and Charles W. McCurdy analyze eminent-domain law as molded by California decisions between 1849 and 1900, and Morton Rothstein describes California's early response to world grain

demands and the growth of an important export sector in the state's economy.

Among other excellent articles is Theodore Saloutos' appraisal of immigrants as California tenants and farm owners in the 1880-1940 era; another is the interesting and thoughtful piece by Walter J. Stein on the "Okie" as a farm laborer, a study of adjustment to the unfamiliar "factories of the field" in the Depression thirties.

Space permits citing only the best articles of the volume; the remainder are more uneven in quality. Inclusion of introductory comments of session chairmen and concluding remarks of commentators add little to the collection. The exceptions to this generalization are the comments by Gerald D. Nash on values and western society, by Earl Pomeroy on law and government policy, and by Rodman Paul on trends and immigrant groups. Previously published as volume 49, number 1 of *Agricultural History* in early 1975, this is a valuable contribution to an understanding of the Far West. What it lacks is a solid introduction that gives an overview and ties together its disparate parts.

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MARGARET W. ROSSITER. *The Emergence of Agricultural Science: Justus Liebig and the Americans, 1840-1880*. (Yale Studies in the History of Science and Medicine, number 9.) New Haven: Yale University Press. 1975. Pp. xiv, 275. \$15.00.

The economic history of American agriculture would have been a very different story had advances and applications in chemistry matched progress in farm machinery. As it was, there were no "green revolutions," and chemical knowledge played a minor role until almost the end of the nineteenth century. Margaret W. Rossiter's interesting monograph on the influence of Justus Liebig shows that the indifference of farmers to the blandishments of science was as much a consequence of the meager fare offered by the scientists as of any ingrained anti-intellectualism on the part of cultivators. Not that American scientists were preoccupied with basic research; they strove to make chemistry relevant and responded to "the changing moods and demands of society . . . by shrewdly stressing its practical application at one moment and then its contributions to pure science at the next." Meanwhile, as in many other reformist causes, "faddish enthusiasm" for applied science coexisted with "outright rejection." But there was so much quackery, such exaggerated puffing of

"new" and costly "aids" to nature, and so little to show for it, that many farmers preferred going into debt for an "improved" machine.

Rossiter focuses her study on the reception of Liebig's work, the craze over soil analysis and the reaction against it, and the transplanting of the chemical research laboratory and experiment station from provincial Germany to the eastern United States. She presents a succinct account of the ascendancy of German scientists in the new field of organic chemistry and directs attention to Justus Liebig. Under Alexander von Humboldt's patronage, Liebig obtained a professorship at the University of Giessen in his native Hesse-Darmstadt. There he built up the celebrated laboratory and went on to publish his *Organische Chemie in ihrer Anwendung auf Agricultur und Physiologie* in 1840. An edition of the English translation came out in 1841 at Cambridge, Massachusetts. Revised and enlarged editions regularly appeared in the United States, along with Liebig's *Familiar Letters on Chemistry*, through the 1870s. But it was in the farm papers, such as the *Albany Cultivator*, that Liebig's work was widely advertised. His shift in emphasis from ammonia to minerals and phosphates in the mid-40s set off the great enthusiasm for soil analysis that led eventually to the "artificial guano" and fertilizer frauds and to the great disenchantment following their exposure by scientists, notably Samuel W. Johnson, Liebig's former Munich student.

Rossiter's major contribution is her account of institution-building with the importation of methods and ideas from the German states. The laboratories established by Eben Norton Horsford at Harvard and by John Pitkin Norton (not a Liebig student) and his father-in-law Joseph E. Sheffield, the railway magnate, at Yale became the leading centers of scientific activity during the decade and a half that preceded the opening of the Johns Hopkins University in 1876. From the Sheffield School, Samuel W. Johnson, Wilbur O. Atwater, and Orange Judd went on to urge the establishment in Connecticut during the mid-70s of a publicly supported "experiment station" after the Saxon example privately founded at Möckern in 1851. Rossiter shows convincingly that the cost of building and staffing laboratories and *Versuchstationen* in the United States was so much higher than in contemporary Germany that the early American institutions were chronically underfinanced.

In the long run, Möckern may have had a greater impact on American agriculture than Giessen. Although there was little basic research done in the United States, the experiment stations became more numerous toward the close of the cen-



tury. The cattle-feeding researches of Wolff had a greater influence on practice than the soil analyses. W. O. Atwater and Henry P. Armsby, both Sheffield graduates, and William W. Daniells were missionaries to the dairy and livestock industries. Rossiter does no more than touch upon this other strand of agricultural science because a fuller treatment, perhaps, would have taken her back through Kühn, Wolff, Grouven, and Thaer to other parts of Germany and away from Liebig and Giessen.

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Stony Brook

GUNTHER BARTH. *Instant Cities: Urbanization and the Rise of San Francisco and Denver*. (The Urban Life in America Series.) New York: Oxford University Press, 1975. Pp. xxv, 310. \$11.95.

It is Gunther Barth's thesis that San Francisco and Denver were "instant cities," American representatives of a type of urban settlement that can also be identified in the colony-cities of the Hellenic world, in the mining and commercial towns planted during the medieval German advance into Slavic Europe, and in political capitals founded by decree (St. Petersburg, Brasilia). The common characteristic of such instant cities is that "they came into existence suddenly and flourished immediately" following the command of a leader or the exploitation of an economic resource (p. xxi). Because these cities became major urban centers in the brief span of a generation, unique patterns marked the lives of instant citizens. Caught in the turbulence of sudden change, the residents "lived in a culture so totally different from the ones they had left behind that often the transition was unbearable" (p. xxiii).

The first third of the book is a prolegomenon to the central illustration of its theme. In chapter 1, "Variations of a City Type," Barth defines the instant city and adduces examples from previous eras. In chapters 2 and 3 he delimits the character of Denver and San Francisco by describing Far Western cities that did not share their common defining traits—the now-vanished market center of Champoege, Oregon, the somnolent Hispanic-American towns of Santa Fe, New Mexico, and Monterey, California, and Salt Lake City, which served as the instant capital of the Mormons. The core of the book consists of 140 closely argued pages on the evolution of Denver and San Francisco. Topical arrangement allows frequent cross-cutting between the cities to illuminate the processes of settlement, the development and maturation of speculative economies, the transplanting of

political and social institutions, and the establishment of cultural identity.

The strength of Barth's study is its condensation of a formidable array of printed journals, letters, travelogues, reminiscences, contemporary histories, and public documents into a rich description of the process of urban growth and the character of society. It is a first-rate example of the historian's art of finding life and pattern in the holdings of library shelves. Barth's portrayal of Arapahoes, Mexicans, and mountain men as actors equal to Anglo-American entrepreneurs in their influence over the early formation of the two cities is one example of a fresh and stimulating perspective. Similarly, his discussion of the way in which new urbanites defined their collective identity offers some of the best insights into the development of urban self-consciousness since the work of R. Richard Wohl, A. Theodore Brown, and Charles Glaab in the late 1930s and early 1960s. Barth convincingly develops the idea that "life styles in San Francisco and Denver were taken as accomplishments of the cities, not specific classes" (p. 183).

Some of Barth's evidence may not be consistent with his thesis about the strangeness of life in instant cities. He states that the culture of these cities involved a "flood of made-to-order imitations of eastern and European models" and that residents "hurriedly adapted transplanted social and political practices to deal with basic organizational problems" (p. 155, 185). In so doing, they followed the precedent established by the founders of earlier Mississippi Valley and Great Lakes cities as much as the dictates of their unique circumstances. Readers will want to draw their own judgments as to whether San Francisco and Denver followed the patterns of institutional imitation and social conservatism outlined by such historians as Bayrd Still, Richard Wade, and Earl Pomeroy.

The major reservation about Barth's study, therefore, concerns the definition of "instant cities" as a unique type. What in actuality is the difference between the growth of Denver (gold rush, ten-year depression, future-oriented boosterism, new prosperity with railroads and silver), and that of Chicago (land boom, ten-year depression, future-oriented boosterism, new prosperity with canal, railroads, and grain trade)? If abruptness of growth is the central distinguishing trait of an instant city, we need more precise and quantifiable criteria to enable us to distinguish which cities followed a "measured beat of growth," which had a "hectic rate of progress," and which experienced an intermediate pace of growth (p. 91). In addition to describing San Francisco and

Denver, Barth's book may contribute to urban history by stimulating further research on the importance of growth rate as a variable influencing the character of urban society.

CARL ABBOTT  
Old Dominion University

JOHN ROWE. *The Hard-Rock Men: Cornish Immigrants and the North American Mining Frontier*. New York: Barnes and Noble. 1974. Pp. x, 322. \$18.00.

Books on the Cornish, rather like books on railroads, often attract an audience on both sides of the Atlantic, whether their authors are amateur enthusiasts or professional historians. *The Hard-Rock Men*, which might be classified as a lively narrative documented by extensive research, attempts to appeal both to Cornish admirers and to an academic audience. Starting with a survey of conditions in the home county, the author then relates both the experiences and the impact of the Cousin Jacks on the many North American mining frontiers. Following a chronological sequence from the lead region of the upper Mississippi Valley, through the copper districts of Michigan, and then out to the gold fields of California, Cornish immigrants left their mark in a variety of ways ranging from mining techniques to place names. Then as the mining frontier turned inland, to the Rocky Mountain areas in the late 1850s and to the Lake Superior region, the southwestern deserts, and the northwestern hills in the 1860s and 1870s, so Cornishmen continued to move into pursuit of one of their historic means of livelihood while contributing to and being influenced by the American environment.

Cornish devotees will doubtless enjoy the details of individual miners' achievements, and Americans with Cornish forebears will delight in gaining more knowledge about their heritage. Students of immigration will also be able to glean information on push and pull factors, the process of cultural adjustment, and the scope of economic opportunity in the United States. For these contributions to enlarging the boundaries of historical knowledge John Rowe must rightly be accorded many thanks. But the lack of a systematic and analytical approach denies the specialist the occasion to attempt useful generalizations. To be sure it is very difficult to identify and to quantify Cornish-Americans, as distinct from English-Americans, but at times the author seems to delight in bypassing recent scholarship on geographical mobility. Preferring a more traditional, and at times almost meandering, approach that sets out the complexity of Cornish-American experiences as seen through the source materials, he conveys a well-drawn descriptive picture. In this he is perhaps not only reflecting his

own depth of feelings for the Cornish, but may well gain the support of that larger public.

MARGARET WALSH  
University of Birmingham,  
England

ELMER R. RUSCO. "Good Time Coming?": *Black Nevadans in the Nineteenth Century*. Foreword by KENNETH W. PORTER. (Contributions in Afro-American and African Studies, number 15.) Westport, Conn.: Greenwood Press. 1976. Pp. xix, 230. \$13.95.

In the introduction to this slim volume, which was a doctoral dissertation written under his direction, Kenneth Porter persuasively argues the need for historical monographs on blacks in each of the western states. Elmo R. Rusco has tried to fill this need. He has labored conscientiously in newspapers and manuscript census records, but the subject is not a rewarding one because of the smallness of the black population and the dearth of source materials. In 1860 there were 60 blacks in Nevada; in 1880 there were 396, and they constituted only 0.6 percent of the population. Many of these settlers came to Nevada via California and continued to maintain ties with the black communities in that state.

Although the number of blacks was miniscule, white Nevadans had racial attitudes similar to those of whites in other states and territories of the Middle West and West, and they imposed the same legal disabilities. Blacks were barred from voting until the adoption of the Fifteenth Amendment. In the years after the Civil War most of the other legal disabilities were removed by the state legislature, but the antimiscegenation law remained. In 1871 the Nevada Supreme Court declared unconstitutional the law requiring that schools be segregated.

In this frontier society a disproportionate number of early black settlers were unmarried males—a characteristic of white settlers also. For the most part the blacks subsisted by doing manual labor. Several were cowboys, and a few became ranchers. Very few were miners, and blacks were apparently excluded entirely from the Comstock mines. There was one black medical doctor, and a handful of small businessmen, mostly barbers. In spite of geographical isolation and small numbers, Nevada blacks made some efforts to establish their own churches and fraternal organizations. They were active politically, and some of them protested eloquently against discriminatory treatment.

Rusco's study concentrates on black-white relations, and he makes only occasional references to Indians and Chinese. His work would have been enriched by a comparison of the status of blacks

with that of other minorities and by more information about the relations of blacks with these groups.

EMMA LOU THORNBROUGH  
Butler University

HERBERT WEAVER and KERMIT L. HALL, editors. *Correspondence of James K. Polk*. Volume 3, 1835-1836. Nashville: Vanderbilt University Press. 1975. Pp. xxxvi, 836. \$25.00.

In the years 1835-36, James K. Polk continued to rise as a national figure in the Democratic Party because of his loyal attachment to President Jackson and his skills as a politician. By 1835, the congressman justifiably believed he was in a pivotal position in a presidential contest between Jackson's preferred successor, Martin Van Buren, and other aspirants, notably fellow Tennessean Hugh White. Although he professed friendly feelings toward White, Polk would not countenance irregularity: Democrats must support the choice of their convention. And White was hardly suitable, in light of his deplorable connections with Nationals (or Whigs, as they were now called), nullifiers, and blue-light Federalists. So Polk zealously campaigned for Van Buren, both from Washington by correspondence and back home on the stump. In doing so, his rivalry with John Bell intensified, with Polk prevailing in his election as Speaker of the House in December 1835. But Bell found some consolation a year later when White carried Tennessee, even though he failed to win the presidency.

The dominant theme of the volume, therefore, concerns the behavior and mentality of a completely committed Jacksonian in the new politics of the thirties. Polk viewed issues and personalities strictly from this perspective, and everything depended upon electoral victory. His letters seldom touch substantive questions of policy or ideology, much less so than one finds in Jackson's colorful prose, while they abound with news and advice on party strategy and tactics. Among his numerous correspondents were his one-time law teacher, Senator Felix Grundy, his congressional colleague, Cave Johnson, and editor Samuel Laughlin of the *Nashville Union*. Like Polk, all were determined to vindicate Jackson and to elect Van Buren, and all relied upon their friend for leadership. The alcoholic and financially pressed Laughlin looked to Polk more anxiously than the others. Little wonder that Polk seemed increasingly less interested in family matters and news from his plantations, though there are many valuable items here relating to those topics.

The editors have again provided a thoroughly professional and usable collection of Polk's corre-

spondence, and readers will look forward to the latter volumes of the series. At that stage, selection will be difficult—in the present work, the editors publish or summarize about seven hundred out of a thousand letters. Perhaps they should have sifted more rigorously and moved closer to the most significant period of Polk's career.

MAURICE G. BAXTER  
Indiana University,  
Bloomington

JOSÉ ENRIQUE DE LA PEÑA. *With Santa Anna in Texas: A Personal Narrative of the Revolution*. Translated and edited by CARMEN PERRY. Introduction by LLERENA FRIEND. College Station: Texas A & M University Press. 1975. Pp. xxix, 202. \$10.00.

A Spanish-language edition of this work appeared in 1955. Carmen Perry obtained the manuscript for translation from the Sánchez Garza family, but no provenance is given for it earlier. The pre-1955 background is important because the work is a critical addition to Texas Revolution sources and because there could be a question about its validity. While it has in general the ring of truth, there are some dubious elements. First, Perry was able to trace José Enrique de la Peña's military records with a fair degree of completeness in Mexican archives, but found none to indicate Peña was assigned to the Texas campaign. Second, the work was clearly intended to besmirch Santa Anna, two of his generals, and the conduct of the campaign in Texas—in short, it is a political tract. Then, the work is purportedly based on a field diary kept by Peña during the campaign, but large segments of it admittedly came from material that Peña gathered later. Fourth, Peña's introduction is dated September 1836, but several entries were clearly made later: a reference is made to an 1838 publication by José de Urrea, and a few other allusions are made to facts that it seems unlikely, although not impossible, that Peña could have known in September 1836. Fifth, Peña was not mentioned in the diaries and other accounts of the campaign, either by his fellow officers or by Texans. Finally, Peña described some particulars of the campaign, especially the surrender of Crockett at the Alamo, not given in any of the other accounts.

Peña's description, however, of the terrain, the stream crossings, the frequent rains, and the interminable mud surely sound authentic, but it is impossible for me to pass judgment on either the authenticity or the reliability of Peña's account. There are enough problems in it to raise serious questions, but it is otherwise filled with details easily corroborated in other works. When the book was issued a few months ago, it was hailed by journalists as, "at last, the real truth about the

Alamo." Alas, it merely adds to the historiographic complexity.

The book is handsome. Llerena Friend's introduction is marvelously scholarly, but the index has shortcomings and the footnotes are sometimes confusing. For example, desertions by both Mexican and Texan soldiers are mentioned frequently, but no index entry was made. Footnotes by Peña, Garza, and Perry are used. Some, which would seem to be Perry's, are not indicated as such.

SEYMOUR V. CONNOR  
Texas Tech University

EDWIN SCOTT GAUSTAD, editor. *The Rise of Adventism: Religion and Society in Mid-Nineteenth-Century America*. New York: Harper and Row. 1975. Pp. xx, 329. \$12.50.

This is a useful book, provided its nature and scope are understood. It is based on a series of lectures at Loma Linda University in which ten scholars explored the social and cultural milieu within which the Millerite movement and the several Adventist denominations emerged. The exploration is wide ranging and does not adhere to a narrow definition of relevance. Thus there are chapters on spiritualism, science and religion, health reform, and communitarianism, as well as on millennialism in general and Millerism in particular.

The scope of the book is indicated by the subtitle; the title by itself would be likely to mislead. The lectures do not, for the most part, break new ground. From this generalization one might except David T. Arthur's chapter on Millerism, which focuses on the crucial role of Joshua V. Himes in helping to transform Millerism from an interdenominational movement into a separately organized sect, and Jonathan M. Butler's chapter, entitled "Adventism and the American Experience," which explores successive phases of Seventh-Day Adventism with respect to the issue of withdrawal from or involvement in political concerns. But most of the lectures are good summaries of materials that historians may find familiar. Thus William G. McLoughlin presents a version of his thesis, presented elsewhere, that America's history reveals a pattern of religious revivals followed by social reformations, and that we are now in the midst of the fourth Great Awakening; and Ernest R. Sandeen gives a distillation of his researches on millennialism that have already yielded *The Roots of Fundamentalism* (1970). It is good to have such summaries of work done and in progress, and they should be of interest to many who are not historians by profession. But this is not a collection of articles from scholarly journals

or an adequate account of the early development of a religious denomination. One may therefore doubt the long-range staying-power of the body of the book. Yet the bibliographical essay at the end will be of lasting value, and many will consider it the most important item in the book.

CONRAD WRIGHT  
Harvard Divinity School

DALLIN H. OAKS and MARVIN S. HILL. *Carthage Conspiracy: The Trial of the Accused Assassins of Joseph Smith*. Urbana: University of Illinois Press. 1975. Pp. xiv, 248. \$7.95.

On June 27, 1844, a crowd of "respectable men" assassinated the Mormon prophet Joseph Smith and his brother Hyrum at the Carthage, Illinois jail. A year later, five alleged leaders of that crowd were tried for murder and acquitted. The odds against their having been convicted were great. The five were clearly supported by public opinion, both in their home county of Hancock and throughout Illinois. Their team of defense lawyers included some of the most eloquent advocates in the state. Against them, Governor Thomas Ford appointed the lonely figure of Josiah Lamborn, once a ruthless and successful prosecutor, but now reduced in powers by drink and dissolution. The defense presented several friendly witnesses who either lied or conveniently forgot the details of June 27. The judge excluded Mormons and their sympathizers from the jury on the grounds of prejudice, but he permitted known anti-Mormons to serve. In the words of the authors, the entire trial was a "quiet perjury . . . to screen a murder."

*Carthage Conspiracy* reminds us that every period of American history has had its sensational trials. National newspapers sent reporters to cover the courtroom drama. Readers were able to follow the witnesses' testimony and evaluate the lawyers' tactics, especially the sparring between prosecutor Lamborn and his chief defense opponent, Orville H. Browning. Dallin H. Oaks, president of Brigham Young University, is a former law professor, and his knowledge of courtroom procedures guides the reader skillfully through the details of the trial. Oaks and Marvin S. Hill paint an engrossing portrait of the due process of law as it was applied on the Illinois frontier.

Beyond explaining the nuances of nineteenth-century criminal justice, *Carthage Conspiracy* raises some serious questions about the ability of jury trials to arrive at justice itself. The authors argue that the Carthage trial was an example of "jury nullification," or a decision by the jury members to disregard both evidence and statutory law in the name of a higher law—in this case, their moral



sense that the Smiths' deaths served the public interest. Jury nullification, Oaks and Hill point out, is a corollary of popular sovereignty. They ask if the majority should be allowed to reign as freely in the judicial world as it does in the political, particularly if its actions result in the cover-up of cold-blooded murder. The jury system has come under attack in our own time for its inadequacies; *Carthage Conspiracy* demonstrates that, in this instance at least, the system worked no better in the 1840s.

MICHAEL FELDBERG  
University of Massachusetts,  
Boston

RONALD J. JENSEN. *The Alaska Purchase and Russian-American Relations*. Seattle: University of Washington Press. 1975. Pp. xx, 185. \$7.95.

This is a concise and judicious account of the origins, negotiation, and ratification of the Alaska Purchase, as well as of the subsequent maneuvers (not excluding bribery) for approval of the appropriation in the House of Representatives. It includes a useful historiographical review, particularly valuable for the author's evaluation of Russian scholarship on the subject.

For the most part Jensen's straightforward text recounts the well-known story of Russia's growing interest in disposing of its Alaskan territory. Russia's inclination to sell appeared as early as the Crimean War, was revitalized in the years immediately preceding the Civil War, and was brought to term soon after the end of that conflict. In the event, of course, the imperial government dangled the bait before expansionist Seward, and he grabbed it quickly. Proponents of the treaty in the Senate and House were moved by the same consideration, nourished by hopes for Asian trade, hopes leavened by the mystique of Manifest Destiny and sweetened by appreciation for Tsarist friendship during the late war.

The value of this account lies in Jensen's providing for the first time since Victor J. Farrar's book of 1937 a complete account in a single volume, using the available Russian sources and the most recent American materials. The book does not seriously alter our view of the episode. Nor does it change the generally accepted judgments on the motivations and activities of the participants. But it adds new insights and interesting detail, based on some Russian materials and American manuscripts not utilized by Farrar. Furthermore, Jensen's efforts to place the event in the broader context of American diplomacy and to analyze the circumstances and personalities at each stage of

the transaction add dimensions not essayed by Farrar.

The book includes an excellent bibliography.

ROBERT PAUL BROWDER  
University of Arizona

ORLANDO W. MILLER. *The Frontier in Alaska and the Matanuska Colony*. (Yale Western Americana, number 26.) New Haven: Yale University Press. 1975. Pp. xi, 329. \$15.00.

WILLIAM R. HUNT. *North of 53°: The Wild Days of the Alaska-Yukon Mining Frontier, 1870-1914*. New York: Macmillan Publishing Company. 1974. Pp. xvi, 328. \$12.95.

Orlando Miller has written one of the more important scholarly books on Alaskan history in many years. A student of the late Richard Hofstadter, Miller brings to his analysis of rural rehabilitation in Alaska the kind of careful inquiry into the relationship between myth and reality that characterized his mentor's work.

Two hundred welfare families from northern Wisconsin and Minnesota were transplanted to the potentially agricultural Matanuska Valley in south-central Alaska in 1935 by Harry Hopkins' Federal Emergency Relief Administration (FERA). This was one of numerous resettlement projects during the New Deal based on the premise that the self-sufficiency of agriculture might help solve the economic ills of the nation while contributing to a redeveloped sense of human dignity. The project was greeted in Alaska with hopeful rhetoric, for development and population seemed to many the territory's most pressing needs.

Appearances often deceive, however, and Miller has written a penetrating study of the differences between the potential imagined and the reality experienced by colonist and planner alike. "Colonists, federal officials, older Alaskans, and journalists all had pictures in their minds," Miller writes, but "those images were touched [but] lightly by reality."

Alaskan conditions made yeoman farming difficult, but, surprisingly, climate was only incidentally responsible. The first major problem was land clearing. The birch and willow that blanket much of the land normally number three hundred to the acre. Even the most dedicated farmer would have needed help under such circumstances, and considerable aid was given by the FERA and by its successor agency, the Alaska Rural Rehabilitation Corporation. Payment for land clearing, for road construction, and for the excusing of credits with the corporation and of millions of dollars in loans made the burden of



colonizing easier. However, this departure from the popular image of frontier self-reliance cost the project support and popularity.

Miller suggests, however, that the fundamental problem lay with the colonists themselves, who were not much inclined to the consistent sacrifice and numbing drudgery required to make an agricultural garden of the wilderness. In addition, many lacked farming experience, contrary to the requirements established for participation. Even had there been a farming tradition among them, however, many would have been, as indeed many were, attracted to the cash incomes of military construction in nearby Anchorage.

In the epilogue, Miller maintains that rural life today has become mainly a hobby. His study would suggest that for all its constituencies—colonist, Alaskan, federal planner, “boomer” journalist—the Matanuska colony was fundamentally a hobby from its earliest beginnings, with frontier mythology quickly succumbing to the hard realities of real pioneering. Though his thesis is too often obscured by his statistics, and his explanation of the bureaucracy that managed them, Miller’s book is a most welcome contribution to serious analysis of the frontier myth in Alaska.

*North of 53°* by William R. Hunt is less successful as a scholarly study. Content to represent the exhilarating chaos of the gold trail, he has written an anecdotal narrative that follows hungry prospectors, canny shopkeepers, hopeful hangers-on, and crafty lawyers through a succession of gold camps and temporary towns stretching north from Skagway to Dawson and Circle on the Yukon, deep in the northern interior, and then west through the flashes of Rampart, Ruby, and Iditarod, to Nome on the Bering Sea. Along the way such luminaries as Jack London, Robert Service, Wyatt Earp, and the Guggenheim brothers emerge as heroes or villains of temporary significance, but romantic proportions.

“Saints and sinners, whores and housewives, swindlers and laborers,” writes Hunt, “alike attempted a hasty adjustment to novel conditions in a land that seemed strange and forbidding.” The relationship between Alaska’s development and its idealization in the minds of those drawn to it, as well as the place of Alaska in the saga of westward movement and the transplanting of civilization, stands in need of a more thorough interpretation than this work provides. Nonetheless, Hunt’s study collects the varied pieces of gold-rush Alaska into a connected set of experiences and, in so doing, invites scholars to give this outpost of culture their careful and serious analysis.

STEPHEN HAYCOX  
University of Alaska,  
Anchorage

JOHN HOPE FRANKLIN. *A Southern Odyssey: Travelers in the Antebellum North*. (The Walter Lynwood Fleming Lectures in Southern History.) Baton Rouge: Louisiana State University Press. 1976. Pp. xvii, 299. \$12.50.

Two points should be made at the outset. This is not an anthology of travel accounts, and the title is misleading as to the scope and period covered. Some of the most interesting material is not directly linked to travel, and, happily, the narrative does not end at 1861.

This volume, the winner of the Jules F. Landry Award, had its genesis in the 1972 Walter Lynwood Fleming Lectures in Southern History. In its present form it is a wide-ranging analysis of North-South contacts down to about 1870 that produced a Southern love/hate relationship. While the South produced no Olmsteds, John Hope Franklin has found that Southerners have left their comments, published and unpublished, on virtually all aspects of Northern life. These he has reported in an even-handed manner. The comments are both favorable and uncomplimentary, for “southerners never spoke in one voice about the North.”

By keeping quoted material to a minimum, the author has eliminated the dullness of repetition. “There is a limit to the ways that one can say that New York is busy and noisy or that the people of Philadelphia are cultivated and cold or that Boston abolitionists are conniving and hypocritical.” Nevertheless, the full vigor and ambivalence of Southern attitudes speak for themselves. Underlying Franklin’s analysis is the question: Could the War have been averted if the sections had known each other better? His answer is that as far as the North was concerned, “to know them is to love them” was not necessarily true in the case of Southerners.

While many Southern reactions were predictable, it is well to have them documented. But it is the unpredictable and unexpected that add spice to the reading. This book reflects the high standards of both author and publisher, and it should appeal to a wide spectrum of readers.

JOHN S. EZELL  
University of Oklahoma

RALPH A. WOOSTER. *Politicians, Planters and Plain Folk: Courthouse and Statehouse in the Upper South, 1850-1860*. Knoxville: University of Tennessee Press. 1975. Pp. xiii, 204. \$9.75.

After studying the lives of state and local leaders and the governmental system of the upper South in the 1850s, Ralph A. Wooster concludes that the South was essentially democratic prior to secession. Massive research in manuscript census re-

turns and county courthouse records uncovered state leaders who were self-made young lawyers and small farmers. Most officials campaigned for office and supported the interests of their constituents. Turnover in office not only provided the opportunity for many men to serve in public life, but it created leadership discontinuity that prevented any interest group from controlling state politics. Through exhaustive analysis of county court cases, legislative proceedings, and the elaborate laws and state constitutions that governed political activities and procedures, Wooster also shows the political system at work. For this analysis all political historians and students of secession are in his debt.

But a historian must also turn process into action. Nowhere does Wooster examine the extent of large planter interests' control over local and state politicians. He fails to reflect on the relationship of state legislation and county court decision to the power of the elite, and he neglects family connections as links to continuous elite power. Rapid turnover in office suggests that leaders with little political experience or ability to respond to constituent interests led the South into war. In this study of leadership and the political system, there is simply no attempt to deal with the larger question of democracy and secession.

In short, Wooster has only delivered the raw materials of political action. He should combine his detailed study of the process and the laws of politics with a larger study of political power and action in the antebellum South. As the author of three important books on antebellum political life, he has certainly earned the right to undertake such a task.

JON L. WAKELYN  
*Catholic University of America*

WILLIAM J. EVITTS. *A Matter of Allegiances: Maryland from 1850 to 1861*. (Johns Hopkins University Studies in Historical and Political Science, Ninety-second Series, number 1.) Baltimore: Johns Hopkins University Press, 1974. Pp. xii, 212. \$11.00.

In past years historians had to cull information about Maryland politics during the era of the Civil War from sectional or national accounts. Recently a few studies have appeared to enrich the historian's knowledge of Maryland in this period. William Evitts adds an excellent study of politics and political loyalties in the border state from 1850 to mid-1861.

Although a political history, *A Matter of Allegiances* is also "a precise kind of social history." Evitts' subject is the populace of Maryland and its changing political alignments. Nevertheless, Evitts discusses key politicians and accounts for the rise and fall of political parties. He also carefully ana-

lyzes the entanglement of social and economic issues with political ones. He shows that the solution to local problems often influenced the response to sectional problems confronting the nation.

While facing national crises arising from the Compromise of 1850, the Kansas-Nebraska Act, John Brown's raid, and the election of Lincoln, Marylanders also confronted local crises. They battled for constitutional reform and voted on a new constitution in 1851, fought for reform and solutions to urban problems in the mid-fifties, and demanded the removal of the entrenched Know-Nothing machine and an end to corruption and violence in the late fifties. Each crisis produced a shift of loyalties and new political alignments. Evitts finds that Whiggery collapsed in Maryland not because of its support for compromise in 1850 but because of its failure to endorse constitutional reform. He attributes the success of the Know-Nothing party to its espousal of nativism and the workingmen's movement. The party declined because of its association with corruption, gangs, and violence in Baltimore, and not because of its appeal for Unionism and silence on slavery. He ascribes the defeat of Constitutional Unionist Bell and the victory of Democrat Breckinridge in the presidential election of 1860 to the lingering resentment over nativism and corruption associated with Baltimore Know-Nothings, and not to Marylanders' pro-Southern allegiance. The real test of the border state's allegiance, Evitts claims, came during the secession winter when Unionism, "always uppermost in Maryland," triumphed. Evitts has contributed measurably to the understanding of Maryland's history. He has provided ample tables and maps to illustrate voting records.

MARY CATHERINE KAHL  
*Towson State University*

CHRISTOPHER DELL. *Lincoln and the War Democrats: The Grand Erosion of Conservative Tradition*. Rutherford: Fairleigh Dickinson University Press, 1975. Pp. 435. \$18.50.

This is a valuable if vexing book. It is valuable because it is the first systematic study of the Civil War faction known as the War Democrats. Christopher Dell, with a great deal of subtlety, recognizes distinctions among wartime Democrats and changes in distinctions with the progress of the war. His scope is wide-ranging, examining political developments in all the loyal states, and moreover he ably connects state and national politics.

The author's argument is arresting. Historians, he asserts, have paid too much attention to the Peace Democrats, neglecting the War Democrats without whose assistance "the federal government would have been powerless to restore the Union by

force of arms" (p. 9). They were led away from true conservatism by Abraham Lincoln, variously described as "the Pied Piper of American politics" and "the butcher of Conservative tradition," who took them down the high road of Radical reform.

The Democrats, in Dell's analysis, divided into three factions: War Democrats, who usually voted for war measures; Conditional War Democrats, who existed only from late 1861 to mid-1863 and who voted for war measures that did not violate civil liberties and property rights of slaveholders; and Peace Democrats, who voted against all war measures.

Yet, despite these classifications, it may be remarked that it is not easy to identify Democrats, Unionists, and even Republicans during the Civil War, and not all historians will agree with the author's determinations. The general argument is further weakened by the vague relationship between Lincoln and the War Democrats, which was, in the author's words, "a compulsory, nervous, neurotic partnership" (p. 10). As for the Radical Republicans, Dell questionably argues that with his Emancipation Proclamation Lincoln bowed to the Radicals and thereafter followed their lead. Dell rides his thesis hard, often filling paragraphs with names of War Democrats in proof of their number and importance and persistently referring to the apolitical U.S. Grant as a Democratic general.

The work is flawed by a considerable number of historical errors, and there is an unacceptably high incidence of typographical mistakes. Although the author drew from a wide range of sources, he made no use of manuscript materials, and he neglected many standard studies. Among these last, from a long list, are Allan Nevins' four volumes, *The War for the Union*, studies of wartime Democrats by Leonard Curry, William Mallam, and David Lindsey, and this reviewer's recent book on Northern politics during the Civil War.

Yet on balance it may be agreed that Dell has gone far toward attaining his aim "to recall the glory of the War Democrats and to emphasize their importance to the country at a dreadful hour when without their aid the country might well have been shot to oblivion" (p. 11).

JAMES A. RAWLEY  
*University of Nebraska,  
Lincoln*

EDWARD G. LONGACRE. *Mounted Raids of the Civil War*. South Brunswick, N.J.: A. S. Barnes. 1975. Pp. 348. \$12.00.

This work is essentially a study of the major activities of the horse soldiers, both Blue and Gray, in

the eastern and western theaters of combat during the years 1862 to 1865. The author examines these raids strategically and tactically and provides a close-up view of this type of military maneuvering. He includes accounts of twelve raids, running the gamut from Stuart's Chambersburg raid of October 1862 (his earlier ride around McClellan's army on the peninsula is treated only briefly) to James Harrison Wilson's march through Alabama in 1864. The book is obviously an outgrowth of the author's earlier articles dealing with cavalry operations and his previously published biography of Wilson. As in the earlier works, Longacre relies heavily on secondary sources, although he makes some use of newspapers and official records.

This book contains little that is new. Longacre is obviously infatuated with the romance that is inherent in past cavalry actions, and he is inclined to glamorize the battles and campaigns. Consequently, the work is essentially drum-and-bugle history, and the author makes little attempt to analyze the social and economic impact of these destructive campaigns on the areas they affected.

For those readers who are not interested in such a book, there is one redeeming quality deserving mention. Longacre provides a great deal of interesting information about the personalities of those he so admires as his heroes in history. Indeed, at times he becomes a garrulous gossip in a rather delightful fashion. For example, in his treatment of General H. Judson Kilpatrick, who helped lead a disastrous raid on Richmond, the author includes the following passage: "Kilpatrick was a complex man. . . . Unlike many members of the Federal officer corps, he neither drank nor gambled; yet certain other vices were dear to his heart, particularly womanizing. Despite his marriage in 1861, he was notorious for his licentiousness, often consorting with camp-followers while campaigning."

The best chapter is the one dealing with the raid led by Colonel Benjamin H. Grierson that was partially responsible for Grant's success before Vicksburg. In a most lucid fashion the author describes the way in which this campaign contributed to the final victory that resulted in the Union forces controlling the lines of the Mississippi.

This book is recommended for Civil War enthusiasts as well as military historians. It deserves to be placed on the shelves of both public and university libraries.

ALVIN R. SUNSERI  
*University of Northern Iowa*

WILLIAM C. DAVIS. *Breckinridge: Statesman, Soldier, Symbol*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1974. Pp. xiii, 687. \$17.50.

While it is hard for those outside the field to agree, there are many aspects of the American Civil War era that lack adequate coverage, despite the immense number of books and articles on the period. Biographies or a revision of older works are needed on at least a score of major figures. Now the young and able historian and editor William C. Davis has perceptively filled one of these voids.

Perhaps because he was on the losing side of the Civil War, was defeated as presidential candidate in 1860, served as vice-president in a lacklustre administration, and was not one of the great military captains of history, John C. Breckinridge has long remained in the shadows. He is always mentioned, but the spotlight has seldom been on him, despite a long career in a number of fields. His name never caught fire.

In this first comprehensive biography of John C. Breckinridge the author has placed his subject in the context of American history. Although perhaps Davis has occasionally drawn Breckinridge larger than life and one might wish for a more introspective approach, this is a sound and worthy volume.

Davis's style is readable and skillful, but his strongest point is probably his extremely thorough research. The large number of manuscripts used includes not only unknown Breckinridge documents and letters, but the papers of the considerable body of men who were associated with him. Newspapers, official publications, and primary and printed sources are well blended. Davis also received considerable help from the Breckinridge family. Unfortunately, there are no maps of his military campaigns.

Davis balances well the lengthy political career of his protagonist in a detailed and fully cited account. In the concluding chapter the author is at his best in a reasoned and noneulogistic appraisal. While sympathetic, he is not afraid to analyze his subject. Perhaps he does not bring out Breckinridge's personal characteristics sufficiently, but this reviewer was impressed by his summary of Breckinridge's career.

Davis feels that Breckinridge's handling of the office of secretary of war in the dying days of the Confederacy was his most lasting contribution to the struggling nation. He has some intriguing statements on the subject. After stating that Breckinridge "was not a strategic or tactical genius," the author says it "would be dangerous to assert that Breckinridge was capable of army command, or perhaps even the leadership of a full size army corps" (pp. 634, 635). This seems sound on the evidence. "The Kentuckian's ability as a general makes all the more difficult the inevitable conclusion that in keeping him in the army the Confederacy wasted his talents" (p. 635). It was "truly

tragic" that President Jefferson Davis did not appoint him to the cabinet post in November 1861, when he considered him for the job (p. 635).

Davis feels that once a decision was reached Breckinridge "was sure he was right, occasionally almost smug, and unyielding in his persistence" (p. 625).

There are many more such appraisals in a volume that must be considered a substantial and needed contribution to Confederate biography. Davis has carried out his statement in the preface: "The search for reasons leads inevitably to men."

E. B. LONG

*University of Wyoming*

LOUISE HORTON. *Samuel Bell Maxey: A Biography*. Austin: University of Texas Press. 1974. Pp. xii, 222. \$10.00.

Samuel Bell Maxey, born in Kentucky in 1825, attended the United States Military Academy and served in the Mexican War. After resigning from the army to become an attorney, he married and moved to Texas in the 1850s. During the Civil War Maxey rose to major general of the Confederacy while serving in the Army of Tennessee and in the Trans-Mississippi Department. In 1874 the Democratic majority in the Texas legislature elected him to the United States Senate, where he served two terms until 1887. As a Senator he concerned himself with a variety of issues which interested his constituents, such as postal routes, Indian affairs, harbor appropriations, railroad construction, and patronage. He unsuccessfully sought an appointment to the Interstate Commerce Commission, but retained an interest in public affairs until his death in 1895.

Maxey appears to have been a competent but not outstanding Senator who failed to achieve the lasting renown which comes to sponsors of significant legislation. Thus the author's statement that Maxey "stood head and shoulders above any other congressman from Texas" during his time in office seems open to debate, especially since he was defeated for re-election by John H. Reagan. The conclusion that Maxey took moderate positions on some sectional issues and was not an extreme Bourbon is more convincing.

Louise Horton has written a traditional biography based primarily upon Maxey's personal papers. She attempts no psychological exploration of his personality, but provides a sound narrative of his public career with glimpses of his family life. Occasionally the background or analysis of military campaigns and political events seems too brief. Yet this volume does contribute to our detailed knowledge of the Civil War in the West and



of government in the Gilded Age. Certainly it will remain the standard biography of Maxey.

ALWYN BARR  
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ADRIAN COOK. *The Alabama Claims: American Politics and Anglo-American Relations, 1865-1872*. Ithaca: Cornell University Press. 1975. Pp. 261. \$13.50.

RICHARD I. LESTER. *Confederate Finance and Purchasing in Great Britain*. Charlottesville: University Press of Virginia. 1975. Pp. xii, 267. \$15.00.

The well-tilled field of Anglo-American relations during the Civil War continues to elicit the talent of able young scholars. Adrian Cook's monograph on the *Alabama* claims and Richard Lester's study of Confederate finance and purchasing in Great Britain are worthy additions to a list including such important recent works as Stuart Bernath's *Squall Across the Atlantic*, D. P. Crook's *The North, The South and The Great Powers, 1861-1865*, Brian Jenkins's *Fenians and Anglo-American Relations during Reconstruction* and also his *Britain and the War for the Union*, and Frank J. Merli's *Great Britain and the Confederate Navy*.

For understanding the crucial Civil War chapter in Anglo-American relations, Cook and Lester elucidate significant aspects in meticulously researched, clearly written, and judiciously executed monographs. Lester (who is not related to me) concentrates on the activities of Confederate agents abroad that led to controversy between Britain and America, while Cook traces the tortuous course of diplomacy leading to settlement after the Civil War. In the attainment of their objectives both Cook and Lester provide a wealth of detail, either heretofore unknown or not clearly focused.

According to Lester, his study was largely written fifteen years ago. That fact probably explains why he does not make much use of more recent writings relating to his subject. This weakness, however, is mitigated by the extensive use of primary sources, principally in British depositories. Lester makes a solid contribution to Confederate history by his masterful analysis of such intricate financial operations as the Erlanger loan and his knowledgeable description of technical matters pertaining to the Confederate cruisers, ironclads, and ordnance. The activities of the Confederate agents, he convincingly demonstrates, played a major role in sustaining the Southern war effort for four years against overwhelming odds. The appendixes are especially useful because of the valuable information they give on the Erlanger operations, the blockade runners, the descriptions of the Confederate cruisers, and the British firms

doing business with Confederate agents. Lester's valuable monograph is marred by a tedious and wooden style of writing, though it might be said that the subject matter does not lend itself to sparkling narrative.

Cook's study, in contrast to Lester's, is written in a more sprightly style and with a surer touch and firmer grasp of his material. An outstanding feature of Cook's writing is his ability to give trenchant evaluations of the dramatis personae in the long controversy over the *Alabama* claims. These evaluations are generally perceptive, though Cook's estimate of Secretary of State Hamilton Fish seems somewhat harsh and his estimate of Charles Sumner is a bit too charitable.

The Anglo-American dispute over the *Alabama* claims involved the sum total of all American compensation demands for Britain's unneutral acts during the Civil War. How to determine the degree of British responsibility, the amount of the claims, and whether they should include the indirect and consequential claims taxed the patience and tested the ability of statesmen on both sides of the Atlantic. Other issues, such as boundary disputes, fishing rights, and commercial privileges, involving not only the interests and sensitivities of Britain and the United States but also Canada, compounded the difficulties. That the *Alabama* claims were finally settled by the first great international tribunal of arbitration was a monument both to the wisdom and tenacity of the negotiators. Their achievement was great in the nineteenth century, but Cook does not overlook its limitation as a pattern for the twentieth. Neither does Cook write diplomatic history in a vacuum. He deftly traces the course of the *Alabama* negotiations against a background of domestic politics and international economics.

The historiography of Civil War diplomacy has been well served by Cook and Lester. This is good fortune indeed for the diplomatic history of the United States because the era of the Civil War was a critical period in Anglo-American affairs. Although relations were destined to improve as bad feelings from the Revolution and the War of 1812 were forgotten and as the *Pax Britannica* waned with the rise of Germany and the United States as great powers, collision over the Civil War difficulties would have had tragic consequences. Failure to settle the Anglo-American difficulties arising from that struggle might well have delayed or even prevented the development of what has aptly been called "the great rapprochement." Such a failure would have been calamitous, not only for Britain and America, but also for Western civilization.

MALCOLM LESTER  
Davidson College



JOE GRAY TAYLOR. *Louisiana Reconstructed, 1863–1877*. Baton Rouge: Louisiana State University Press. 1975. Pp. xii, 552. \$20.00.

Three periods of Louisiana history test the objectivity of historians—the first Spanish decade with its rebellion or revolution of 1768; the reconstruction or military occupation years, 1862–77; and the epoch of “dictator” or mass leader Huey P. Long, 1928–35. The early studies of Louisiana Reconstruction by Ficklen (1910) and Lonn (1918) have been criticized by revisionists, whose studies in turn have not found favor with other historians. Each group has been accused of prejudice. “The tale,” Joe Gray Taylor writes in his introduction, “has never been told as a whole. . . . The study that follows is an attempt to remedy this situation.”

Eight chapters and the epilogue of this volume detail the political narrative of a period so complex that constant character identification and event organization are practically impossible. Three chapters span the typical topics of economic, social, and cultural history in lesser detail. A more exhaustive description of the face of Louisiana at the close of the Civil War would have made possible a broader and clearer understanding of the entire period, as would have a more detailed coverage of two black groups—the former free Negroes of the slaveholding planter class and that band of talented and educated New Orleans free Negroes that in late antebellum years had vaulted black culture to its highest level of achievement anywhere in the world.

Taylor’s volume is the most broadly researched, in-depth account yet written of Louisiana Reconstruction. Specific factual errors are minimal for a work of such extensive coverage, but upon occasion rumor is accepted as fact, opposing points of view or divergent descriptions (even in the same source) are occasionally overlooked, and broad generalizations and derogatory allusions are made without qualification or sufficient caution. The author states that Ella Lonn “definitely sympathized with the Conservatives” (p. 492); some readers of *this* book may be of the opinion that the definitive chronicle of those Louisiana years must await another time, when a succeeding generation will be given “an opportunity to accomplish what their ancestors had failed to achieve” (p. 508).

EDWIN ADAMS DAVIS  
*Louisiana State University,  
Baton Rouge*

*Doolittle Committee in the Southwest, 1865*. Boulder, Colo.: Pruett Publishing Company. 1975. Pp. viii, 85. \$9.95.

In 1865 Congress responded to increased budgetary requests by the Indian Office and War Department for dealing with the Western Indians by appointing a joint committee to investigate the Indian situation. Subcommittees were then appointed to visit and investigate major areas of Indian concentration in the Western United States. The subcommittee assigned to Oklahoma, Kansas, Colorado, New Mexico, and Utah was composed of Senator James R. Doolittle of Wisconsin, Senator Lafayette S. Foster of Connecticut, and Representative Lewis W. Ross of Illinois. This subcommittee, escorted by a contingent of the U.S. Army commanded by General Alexander McDowell McCook, visited Kansas, Colorado, and New Mexico in the course of its hearings and investigations.

After the subcommittee had returned east a series of nine articles appeared in the *Leavenworth Daily Times* under the name of Burwell. The *Chronicle of a Congressional Journey* is a collection of the Burwell articles. The editor surmises that the author of the articles was surgeon Samuel B. Davis, who accompanied the expedition.

The activities of the subcommittee have been virtually unknown, and Burwell’s scribbles do little more than give us an outline of the itinerary of the subcommittee. In large part the *Chronicle of a Congressional Journey* is a rather routine travel journal that is quite unremarkable in the information which it contains. As a member of the expedition, rather than a participant in the subcommittee’s work, Burwell obviously was either uninformed as to the work of the committee or did not trouble to address that work in his writings. His apparently second-hand description of the Sand Creek site a few months after the massacre there is of some interest, as are his condemnation of Colonel J. M. Chivington and his descriptions of Lucien Maxwell, Dick Wootton, Kit Carson, and Santa Fe. Altogether, however, this routine travel journal adds few insights to the already numerous journals that have been published. With only sixty-three pages of text, and a total of ninety-three pages in all, this is an expensive book with a limited content that can probably be best mined by most readers at a library.

BRIT ALLAN STOREY  
*President’s Advisory Council on  
Historic Preservation*

LONNIE J. WHITE, edited with an introduction and epilogue by. *Chronicle of a Congressional Journey: The*

JAMES C. MOHR, editor. *Radical Republicans in the North: State Politics During Reconstruction*. Baltimore:

Johns Hopkins University Press. 1976. Pp. xvi, 200. \$11.95.

Until recently, the history of American society and politics during the Reconstruction era has focused primarily upon the presidency, Congress, and the South. In 1969 the Johns Hopkins University Press published a pioneering volume on Reconstruction politics in the border states; and now Johns Hopkins has published this seminal book on state politics in the North during Reconstruction. Under the able editorship of James C. Mohr, this collection of essays deals with nine of the twenty northern states and succeeds in broadening once again the perspective in which the Reconstruction era must be placed. This book is must reading for anyone interested in the postwar era and is certain to be widely used in both graduate and undergraduate courses in the field. In fact, the study of Reconstruction will never be the same again.

Of the nine essays included in this book, only David Montgomery's piece on Pennsylvania politics has appeared in print previously. The remaining authors and state essays are: Richard H. Abbott on Massachusetts; George M. Blackburn on Michigan; Felice A. Bonadio on Ohio; Richard N. Current on Wisconsin; Robert R. Dykstra on Iowa; Mohr on New York; and Philip D. Swenson on Illinois.

Each essay is a self-contained unit, but there are a number of common themes. Among these are: the omnipresent race issue in American politics; the problem of factionalism, i.e. the Liberal Republican movement, which wreaked havoc during the postwar years; increasing governmental activism at the state level; the continuing importance of ethnocultural political patterns; the relationship between post-Civil War politics, corporate interest groups, and the rise of organized labor; and the crucial role played by courts at the state level.

One hopes that this book will appear in a paperback edition in the near future.

RICHARD ORR CURRY  
University of Connecticut

DON W. WILSON. *Governor Charles Robinson of Kansas*. Lawrence: University Press of Kansas. 1975. Pp. ix, 214. \$11.00.

Charles Robinson played a leading role in Kansas' antebellum struggle, and he served as governor of the free-state territorial faction. Although he was elected as a Republican in 1860 to be the state's first governor, he had by 1862 irrevocably severed his connection with that party, and he never again held a major political office. But he was far from inactive the remaining thirty years of his life, remaining influential in many cultural, economic,

and political activities. As Don Wilson notes, "He was involved in every major reform movement that occurred in Kansas between 1865 and 1890" (p. 101). He was a person of multiple talents and strong convictions, truly a complex human being, and certainly a challenging and worthy biographical subject.

Wilson recognizes that Robinson's "Kansas career had two distinct phases" (p. 167). The first encompassed the years from 1854 to 1862, and the second from 1863 to his death in 1894. Wilson treats the first phase adequately; one hundred pages are devoted largely to this eight-year span. The territorial conflict and the early factional struggle, as well as Robinson's rivalry with James K. Lane, are persuasively handled. Robinson's role receives the favorable treatment it apparently deserves; Lane emerges as the political scoundrel that he evidently was. It is Wilson's contention that Robinson adhered to a "pragmatic political philosophy" in this first phase, which he abandoned in the second to become an advocate of radical reform. This interpretation is not convincing. One suspects that there is far more continuity in Robinson's stands than indicated. It is unfortunate that the later period is not examined more carefully and in a broader context, for one cannot help but feel that therein lies the answer to the apparent Robinson riddle. It will not do to note, but to leave unexplained, the assertion that "the Republican party was his constant foe" (p. 168) in the years after 1863.

This study is flawed, particularly its post-gubernatorial segment, by a serious case of tunnel vision. Indeed, the author is largely oblivious to the broader currents of change at work in American society, and the bibliography is at least five years out of date.

GENE CLANTON  
Washington State University

JAMES M. MCPHERSON. *The Abolitionist Legacy: From Reconstruction to the NAACP*. Princeton: Princeton University Press. 1976. Pp. xiii, 438. Cloth \$20.00, paper \$12.50.

This volume, a worthy sequel to the author's *Struggle for Equality*, takes the story of white abolitionists and their descendants from the latter years of Reconstruction up to the founding of the NAACP in 1910. James M. McPherson bases his account "on the premise that not only did former abolitionists provide much of the leadership for postemancipation racial reform, but also that their attitudes provide a perspective for viewing the successes and failures of those efforts." In challenging "the prevailing assumption that most abolitionists abandoned the battle for Negro rights

after 1870," he seeks to establish that there was an ongoing tradition of white racial egalitarianism that survived the disappointments of Reconstruction and the later onslaught of an intensified Negro-phobia.

The study is divided into three parts: the first chronicles abolitionist participation in the debate over the decline and fall of Reconstruction and shows that a majority of former abolitionists hewed to the "radical" position on issues involving the rights of the freedmen; the second recounts the postemancipation campaign for black education in the South, an effort initiated and strongly influenced by men and women of abolitionist background; the third deals with the revival of militancy as a reaction to the failure of education to stem the tide of racial discrimination in the period from 1890 to 1910. Particularly valuable and illuminating is McPherson's discussion of how abolitionists confronted such troubling developments as the apparent failure of the freedmen to live up to white middle-class expectations during Reconstruction, the growth of irresistible pressure then and later for religious and educational segregation in the South, the rise of a movement in the 1880s and 1890s for black control of the schools and colleges that abolitionists had established, and the coming to prominence at the turn of the century of Booker T. Washington and his philosophy of racial accommodation. McPherson is basically sympathetic to the response of his subjects to these and other challenges, but he does not ignore their shortcomings. Although he absolves most abolitionists of racism, he acknowledges their tendency toward an ethnocentric paternalism that at times hindered their efforts to work with, and not just for, blacks. He describes much of their activity, especially in the area of education, as an effort to achieve egalitarian ends by paternalistic means.

Although the book comes very close to being a definitive study, there is one important question that remains unanswered. Were the 284 abolitionists and neo-abolitionists whose views form the basic data for this work representative of a significant body of post-Reconstruction white opinion on the status of blacks in American society? Or were they merely a sect, whose attitudes were, for the most part, influential only within their own limited circle? In other words, was there substantial white support for their views, or were they talking mainly to each other? McPherson's genealogical approach tends at times to reinforce the latter impression by implying that racial egalitarianism was the preserve of few families with peculiarly strong antislavery antecedents. But elsewhere there are suggestions that white egalitarianism remained a major intellectual tradition even in the age of Jim Crow. So there is still a question

whether "the abolitionist legacy" should be viewed as a minor exception to the larger history of white racism in this period or as a countercurrent of sufficient importance to force us to revise the prevailing view that there was an overwhelming consensus in favor of white supremacy.

GEORGE M. FREDRICKSON  
*Northwestern University*

ERIC H. MONKKONEN. *The Dangerous Class: Crime and Poverty in Columbus, Ohio, 1860-1885*. Cambridge: Harvard University Press. 1975. Pp. ix, 186. \$13.50.

This is an advanced quantitative study of the interrelationships of crime, poverty, urbanization, and industrialization in Ohio. It contains relatively few notes and no bibliography. Its strength derives from the seventy-odd tables, figures, and maps that form the base of a generally lucid and well-reasoned book. The text is liberally sprinkled with provocative interpretations, speculations, and educated guesses.

Eric H. Monkkonen examines the hypothesis that urbanization and concurrent industrialization cause poverty and crime. If this hypothesis has any universality, it can be tested in any growing industrial city; Monkkonen chose Columbus, Ohio. While cautioning against any simple causal relationship and often applying engrossing and brilliant arguments to test the hypothesis, the author finds that the evidence does not support the hypothesis. Neither was there extensive proof that "poverty caused by employment shifts due to industrialization lead to crime . . . if anything crime led to poverty more than poverty led to crime." Moreover, a study of recidivism indicated that "urban and industrial growth diminished the number of career criminals."

In a positive vein, the study shows that urbanization "affected the kind and quality of social deviance." By 1885, when Columbus had attained urban and industrial growth, fraud, embezzlement, forgery, and theft by false pretence—subtle and complex offenses, not violent ones—had emerged as characteristically urban crime.

Monkkonen questions much conventional wisdom on crime and poverty. For example, he comes down hard on the Malthusian and elitist minister from Columbus, Washington Gladden. Charging that Gladden knew little if anything about the poor people themselves, the author concludes that "impoverishment may have had the opposite context than the creation of large families, may have broken them up, and made even small ones impossible." We learn also that "poverty was more responsible for rural crime than it was for urban crime . . . the urban poor were underrepresented in the criminals, the rural poor . . . highly over-

represented." This book sent me scurrying back to James Q. Wilson's recently published, controversial *Thinking about Crime* to search in vain for some indication that perhaps it was sparked in part by Monkkonen's research.

The quantifiable data used by Monkkonen were taken from published United States census reports; *Annual Reports* of the Secretary of State of Ohio; the criminal court ledgers and infirmary records of Franklin County, Ohio; the manuscript census and city directories of Columbus; and the like. Of a highly technical nature, this new social history, enhanced with an obvious interdisciplinary expertise, will be of interest mainly to historians and sociologists concerned with urban-rural problems and relations, crime, and to a lesser extent immigration and ethnicity. Those persons interested in women's history will especially appreciate brief sections dealing with women workers and offenders.

To the majority of historians not trained in the new methodology, the reading will undoubtedly be slow going. But there are optional parts, depending upon one's interests and abilities in source problems and methodology. Still, even the cliometricians will have to read closely with calculators ready. The presentation and discussion of figures are generally clear, although there are exceptions. In one very important case, for example, the reader is told that one can tell at a glance from figure 1, "Ohio Crime Rates," that most crime rates were stable with the exception of statutory offenses, which dropped, and rape, which increased slightly. Monkkonen adds that "these findings run contrary to all published studies of nineteenth century crime" (p. 29). In fact, the issue remains clouded even after scrutinizing the pertinent graphs because they appear to signal a variety of movements in the data rather than a stable trend.

As with other recent books of this nature, the strength of the data and the accuracy of interpretation will undoubtedly be put to the test. The true value of the study may take years to fathom. My guess is that it will be substantial. Those who make the effort to read the book now will be richly rewarded.

LUCIANO J. IORIZZO  
State University of New York,  
College at Oswego

MAXWELL WHITEMAN. *Gentlemen in Crisis: The First Century of the Union League of Philadelphia, 1862-1962*. Philadelphia: The Union League of Philadelphia. 1975. Pp. xii, 386. \$10.00.

The elite civic-social clubs that once marked the urban scene are institutions deserving attention by

city historians and sociologists. In *Gentlemen in Crisis*, Maxwell Whiteman focuses on the Union League of Philadelphia, one of the more successful and long running clubs of this genre. The League was founded in 1862, emerging out of the turmoil within Philadelphia high society engendered by the Civil War. The League became a major force countering the Copperheads and motivating Philadelphians to rally around the flag. It raised nine military regiments, financed the publication of dozens and the printing of thousands of Unionist tracts, and sponsored patriotic rallies and parades. In 1864 the club used its organizational and propaganda skills to help secure the re-election of President Lincoln.

In the post-Civil War era, the League became a major civic and social outlet for the city's elite, with its club life contributing much to the culture of the burgeoning metropolis. The League also forged an informal alliance with the Republican party, regularly endorsing GOP state and national candidates and providing platforms and financial support for their campaigns. It rarely dabbled in municipal politics, although Republicans regularly controlled city hall. In 1903, Lincoln Steffens labeled Philadelphia "corrupt and contented," and Whiteman admits the League did little to disturb the status quo. Its strength was not dedicated to progressive era municipal reform. Rather, the League promoted a protective tariff, the gold standard, and other stock items of old guard Republicanism.

Whiteman's history of the Union League's second half-century, 1903-62, is primarily a chronicle of organization activities and personages, accompanied by a running summary of United States history. At times the author tries to cull from League reports and speeches the perspective of the Philadelphia elite on national trends. Except for such commentary, these chapters are of peripheral interest to the general or urban historian. This evaluation does not hold for Whiteman's treatment of the League's early history. In the book's first section he skillfully weaves data from memoirs, letters, and League files to provide insights into a significant, but neglected, institution of nineteenth-century urban America.

RICHARD J. CONNORS  
Seton Hall University

G. N. SEVOST'YANOV, et al. *Materialy pervogo simpoziuma sovetskikh istorikov-amerikanistov (30 noiabria-3 dekabria 1971 g.)*, in two volumes. [Materials of the First Symposium of Soviet Historians on American History (November 30-December 3, 1971)] Moscow: Izdatel'stvo "Institut Vseobshchei Istorii Akademii Nauk SSSR." 1973. Pp. 276, 266.



The first symposium of Soviet historians of the United States, held in Moscow in 1971, demonstrates the quantitative growth of *Amerikanistika* in the past two decades, the increasing sophistication of its practitioners, and the range and variety of their work. This record of their four-day meeting gives the impression that the very magnitude of the enterprise in which our Soviet colleagues are engaged in large and growing numbers has led to a less monochromatic and more diversified, less political and more scholarly treatment of American history. For some of the forty speakers or discussants whose reports and comments are included in these two volumes, this diversity was cause for concern and a sign of theoretical shallowness; for a majority it appears to be a source of professional pride.

In a lengthy introductory report, "On the Study of the History of the USA in the USSR," G. N. Sevost'ianov, head of the American-Canadian section of the Institute of General History, Academy of Sciences, expressed satisfaction with what had been achieved, pointed to areas and topics in need of further work, and defined the task of students of American history to be "the illumination of the law-governed processes and the specific peculiarities of the development of American capitalism, especially in the epoch of imperialism, and the demonstration of the unsoundness of the theory of American exceptionalism." His injunction not to lose sight of general laws while discovering that which is unique or different in the American experience, must give rise to tensions, fruitful as well as inhibiting, in the analysis of America's past and present.

Some of that tension surfaced at the symposium. In the discussion of Sh. A. Bogina's paper, "On the Role of Immigration in the Ethnic Development of the American Nation," M. M. Malkin, a student of the Civil War period who described himself as belonging to the older generation, chided Bogina for overvaluing the ethnic factor and for not mentioning the formation of a national market and the growth of a national community of economics, language, and sentiment. Only if they were guided by the scientific methodology of historical materialism, Malkin warned, would Americanists avoid superficial descriptiveness, eclecticism, one-sidedness, and the "absolutization" of isolated aspects of reality.

As more and more Soviet scholars gain access to American archives (the archival basis of recent work was praised repeatedly) and as they extend their investigations to new fields suggested at this conference—e.g., regional and local studies, the role of various political and social forces in the making of foreign policy; the history of ideas and of religion, of non-working class radicalism, of

"bourgeois reformism," of liberal and conservative movements and their ideology and politics—the integration of their findings into an overall framework will provide ever greater challenges and stresses, as well as intellectual stimulation on both sides of the ocean.

It is for such stimulation, and for what they tell us about the state of the art in the USSR, that these volumes are valuable, rather than for presenting research results. Most of the formal papers did not, in fact, do so and were of a broader, more generalizing kind. Mention should be made of P. B. Umanskii's survey of the treatment of the American Revolution in the writings of the New Left (critical but largely positive) and of the New Economic History (critical and wholly negative), and of V. K. Furaev's calling attention to neglected aspects of Soviet-American relations, particularly in the years before formal diplomatic relations began. There is even a suggestion (not without a rejoinder) that America's role in the anti-Soviet intervention was retrospectively magnified by the alarms of the Cold War. Finally, there is N. N. Bolkhovitinov's excursion into Russian and American archives. For anyone working in the field of Russian-American relations he is an indispensable resource and guide.

HANS ROGGER  
University of California,  
Los Angeles

VICTOR GREENE. *For God and Country: The Rise of Polish and Lithuanian Ethnic Consciousness in America, 1860-1910*. Madison: State Historical Society of Wisconsin. 1975. Pp. x, 202. \$17.50.

Victor Greene's careful analysis of the historical process by which immigrant Poles and Lithuanians become aware of themselves in America is an important contribution to our understanding of an elusive topic. Considering their Roman Catholicism as worthy of study on its own terms, he argues persuasively that the roots of ethnic self-identification lay mainly in conflict over religion and nationality within the Polish and Lithuanian communities. The Irish or Germans who dominated Catholicism in America, far from acting as oppressors of Polish group feeling, frequently tolerated and occasionally intervened in behalf of Polish aspirations. More often these outsiders were victimized by ignorance or manipulated by intractable partisans struggling for control within the communities. This approach is a refreshing departure from the habit of looking upon turn-of-the-century immigrants as objects of assimilation or bearers of gifts to the melting pot. It takes advantage of non-English language sources and



the studies of specialists in the last decade that have transformed our understanding of the great migration from East Central Europe.

One wishes only for a more intensive analysis of the Lithuanian experience and of the religious culture of Polish peasants comparable to the description of their socioeconomic position. The appointment of the Polish cleric Paul Rhode as auxiliary bishop of Chicago in 1908, it might be added, did not resolve issues within the community as much as the participants hoped. The years that followed were marked by rapid growth in the Polish National Catholic Church and by persistent efforts for representation in the American Church, which continue to this day.

A more troubling question, which Greene deals with briefly, is the degree to which these conflicts impinged on the thinking of most immigrants. Certainly the leadership was preoccupied with issues of ethnic identity in America, and their behavior affected many others. An impressive number of people were involved in public demonstrations and in the formation of institutions with an ideological orientation. Nevertheless, we may be dealing with a minority within a minority. The circulation of newspapers and the membership of ideologically oriented institutions added up to much less than a majority of the immigration. I would have preferred either a more modest hypothesis or more evidence to support the view that "rising ethnic consciousness" in "every corner of Polish and Lithuanian society" was linked to "the manifest persistence of a pervasive intragroup tension."

FRANK RENKIEWICZ  
University of Minnesota,  
Twin Cities

EDWARD PINKOWSKI. *Pills, Pen & Politics: The Story of General Leon Jastremski, 1843-1907*. Wilmington, Del.: Captain Stanislaus Mlotkowski Memorial Brigade Society. 1974. Pp. 172. \$5.95.

At the very beginning Edward Pinkowski proclaims that his book is "not a definitive biography" but "a preliminary look at Leon Jastremski." The author unfortunately is true to his word. Although Pinkowski proceeds with enthusiasm, the fruit of his labors is an uncritical biographical sketch of a nineteenth-century Louisianian who deserved better.

The son of a Polish expatriate and a French aristocrat, Leon Jastremski was born in Soulan, France. At age six he emigrated to the United States and settled with his family in Vermilionville (Lafayette). A printer's devil and drugstore clerk, he joined the Louisiana Volunteers when the Civil War erupted. During the conflict the young cap-

tain suffered wounds to his arm and throat and was captured three times.

After the war Jastremski operated a drugstore with his brother. He also became foreman of a volunteer fire company and joined the Knights of the White Camellia. Both activities pointed toward politics. In 1876 he won the votes of loyal Democrats to become mayor of Baton Rouge, a post he held for three terms. Acquisition of a newspaper only increased his political influence. Jastremski later became a delegate to the 1879 constitutional convention, a state printer, the chairman of the State Democratic Committee, an adviser to Grover Cleveland, and a principal founder of the United Confederate Veterans in Louisiana.

After a stint as United States consul in Callao, Peru, during the second Cleveland administration, Jastremski returned to Louisiana. In 1904 he was an unsuccessful reform candidate for governor. In 1907 he again entered the race, but death cut short his campaign.

Jastremski's diverse career receives scant attention. The author, a student of Polish-American immigration, focuses primarily on ethnic affiliations. Emphasis on Jastremski's Polish heritage, however, is at best dubious. Despite his Polish surname, Jastremski was a native of France, lived mainly in French Louisiana, and wrote commonly in the French language. Lengthy references to Polish immigrants who evidently never knew the Louisianian only cloud the issue. Pinkowski may have rescued Jastremski from historical obscurity, but he does little more.

EDWARD F. HAAS  
Centenary College of Louisiana

LEE SOLTOW. *Men and Wealth in the United States, 1850-1870*. (Yale Series in Economic History.) New Haven: Yale University Press. 1975. Pp. xx, 206. \$12.50.

For many years Lee Soltow's scholarly interests have been focused on the distribution of income and wealth in various western societies during the last 200 years. This volume is the latest and most important of a distinguished series of essays and monographs on these subjects that he has produced. It derives from a careful analysis of three large samples drawn from the manuscript censuses of 1850, 1860, and 1870. Soltow has supplemented these data with other types of quantitative evidence that allow him to treat the evolution of wealth holdings from 1790 to the 1960s. Thus the scope of the work is much wider than the title indicates.

The size of the book—206 small pages—is also a little misleading, suggesting a short afternoon's reading. In all of Soltow's work the ratio of ideas to

words is very high, and in this case he has outdone himself. The book is about as dense as a piece of well-cured oak, and as tough to get through. It will also probably turn out to be as durable as oak. Certainly it should be read by every scholar with an interest in wealth distribution, and it has much information that will be valuable to others as well. The place to begin is with William Parker's foreword. Next, read chapter 7 (the summary), and then proceed to the body of the work.

The chief lesson of Soltow's research is: *plus ça change, plus c'est la même chose*. The broad contours of wealth distribution changed little down to the Second World War, when inequalities were reduced for a period of a decade or so. But changelessness at the macroeconomic level resulted from offsetting shifts at the microeconomic level. The tendency toward greater concentration, arising out of alterations in the structure of the economy, was compensated for by the drift toward greater equality within the subdivisions of the economy.

There is much more, of course, including some analyses and interpretations with which reasonable scholars may disagree. Nonetheless, Soltow has made a major addition to our knowledge of his subject.

ROBERT E. GALLMAN  
University of North Carolina,  
Chapel Hill

STUART D. BRANDES. *American Welfare Capitalism, 1880-1940*. Chicago: University of Chicago Press. 1976. Pp. ix, 210. \$12.95.

This is an admirably compact and comprehensive treatment of the varied company-sponsored programs that emerged to meet the social needs of employees, while simultaneously promising employers relief from troublesome problems and costs stemming from worker indifference, absenteeism, drunkenness, strikes, violence, and unionization. Stuart D. Brandes recognizes that a substantial element of idealism (even if sometimes misguided) as well as calculation entered into the movement, which he believes reached its heyday in the years immediately preceding the New Deal. His assessment of motives, programs, and their consequences is a balanced one although his conclusion is clearly that most company welfare programs had demonstrated their inadequacy even before the collapse of 1929 swept many of them onto the historical ash-heap.

After analyzing the historical context, motives, leadership, and ideology behind the employee welfare movement, Brandes devotes a series of chapters to each of the major categories of programs: housing, education, religion, recreation, profit sharing and stock ownership, medical care, pen-

sions, social work, and employee representation. He has worked through a mass of company records, government reports, personal records, and contemporary and scholarly treatments to excellent effect. What emerges is a summary so solid and complete that, although details may be modified and interpretations varied, it is unlikely that early welfare capitalism in America will need another such study. This is as welcome as it is long overdue.

Some shortcomings, however, suggest themselves. In his commitment to comprehensiveness and conciseness, Brandes has digested quantities of data into a work that is unfortunately bland, impersonal, and homogenized. This does considerably less than full justice to his subject, which includes, as he recognizes, some notably colorful, quirky individuals and undertakings. In another regard, he recognizes parallels, even if incomplete ones, between the welfare capitalist and Progressive movements as they developed over roughly the same period of time. Yet he seems to believe that the New Deal, at least after the Wagner Act, brought a virtual end to employer-sponsored welfare programs since government and unions had stepped in to do what the companies had proved incapable of achieving. On his own evidence, however, this report of death seems rather premature. Had he chosen to do so, Brandes could surely have discovered an array of company-sponsored or -financed welfare programs flourishing today that, in range and variety if not in comprehensiveness, might well outmatch their most ambitious predecessors of the twenties. When to these are added private welfare agencies and government programs, over both of which business leaders have continued to exercise more than incidental influence, there remains a real question whether American welfare capitalism died in 1937 or is alive and well and flourishing in the nation's bicentennial year.

MORRELI HEALD  
Case Western Reserve University

DANIEL NELSON. *Managers and Workers: Origins of the New Factory System in the United States, 1880-1920*. Madison: University of Wisconsin Press. 1975. Pp. x, 234. \$15.00.

This book is not so much an integrated account of the evolution of the factory system of production in America as a series of essays on various topics dealing with the utilization and management of industrial labor between 1880 and 1920. Nelson first provides an overview of the changes in industrial structure during the course of the nineteenth century. With a second chapter on the factory environment, the author covers familiar ground

related to the growth of industrial establishments, specialization of work task, increasing mechanization, new power sources, and the alteration in the physical characteristics of the factory workplace. Nelson also deals with the role of the foreman in the nineteenth-century factory and the system of inside contracting that subsequently gave way under the impact of scientific management. A subsequent chapter concerns the recruitment of the factory labor force, first by informal means such as the hiring of applicants at the factory gate by the foremen, later by means of more elaborate devices such as the company town which settled the labor force and reduced turnover, and, finally, by familiar modern devices featuring specialized personnel departments.

Welfare work as practiced during the period embraced such activities as community services (company-sponsored schools, hospitals, churches, etc.), improvement of physical working and living conditions, and social and recreational outlets for workers. These welfare programs which grew up more or less spontaneously during the Progressive Era also coincided with the wave of state labor laws affecting the factory environment during the first decade of the twentieth century. The combined impact of these voluntary and imposed solutions to various labor problems provided the principal impetus for the centralized, hierarchical industrial administration that emerged in American factories on the eve of the First World War. The final chapter outlines the growth of the American labor movement before 1920 and shows how nascent industrial unionism shaped management response in the factory context.

Though all the topics treated by Nelson are significant and interesting, they fail to come together to provide the reader with a comprehensive understanding of how the separate technological, economic, legal, and political forces combined to affect the evolution of the factory system or of the impact that system had on American life generally. Certain positions adopted by the author appear to be questionable. He argues that the rise of the modern factory system undercut the power of the old-style line foreman vis-à-vis the worker. Certainly such a generalization is far too broad to have meaning within the context of specific workplace settings over time. Here the author's evidence is simply inadequate to support the assertion. In other areas the reader is required to accept on faith the fact that Taylorism had a measurable impact. The author has failed to devise appropriate measures of "impact" and "effectiveness" that are, even in principle, testable. The mysterious appearance of major changes in the industrial environment, such as company-sponsored welfare programs, goes largely unexplained from an analytically satisfactory point of view.

My general unhappiness that Nelson does not integrate the forces and factors affecting industrial evolution in this important period by critical examination and evaluation of the historical materials at hand, is offset by the fact that his book chronicles some important issues in the rise of the factory system and provides us with much-needed new information.

PAUL USELDING  
University of Illinois,  
Urbana-Champaign

SILVANO M. TOMASI. *Piety and Power: The Role of the Italian Parishes in the New York Metropolitan Area, 1880-1930*. New York: Center for Migration Studies. 1975. Pp. xi, 201. \$9.95.

Silvano M. Tomasi's book, originally his doctoral dissertation in sociology, examines the experience of first-generation Italian immigrants in an alien and inimical society. Rejecting the melting pot thesis that immigrants assimilated as individuals, Tomasi believes that Italians entered American society as self-aware groups that were organized around their Italian ethnic parishes and ready to engage in the power plays of a pluralistic nation.

Coming preponderantly from southern Italy, with identities defined by family, village, and parish church, the immigrants had little initial conception of their nationality. In America, harsh conditions and discrimination drove the peasant newcomers to depend upon their native culture, including their religion with its familiar saints and local festivals. But poverty and the opposition of an Irish-dominated Roman Catholic clergy obliged these fragmented village and regional groups to combine to achieve worship in their own language with sympathetic priests. From this struggle emerged the Italian national parish as opposed to the customary territorial parish of the Roman Catholic Church. Conservative American prelates reluctantly sanctioned this institution, divisive as it might be within the church, rather than lose the immigrants. In the process, Tomasi concludes, provincialism gradually gave way to a new Italian ethnic solidarity based upon an awareness of nationality. Italian parish priests, linked with a Catholic Church national in scope and pluralistic in makeup, helped to ease the introduction of Italians into American society "where belonging to a people or to an ethnic group was a condition for adjustment and mobility" (p. 128).

Basing his research upon Catholic archives both here and in Italy and drawing on other English- and Italian-language materials, Tomasi utilizes data not always easily accessible to other scholars. This gives the book a broader application than the subtitle suggests. His use of extended quotations, a common weakness of dissertations, becomes a

source of strength, affording a sampling of Catholic opinion during such critical episodes as the debates over Americanism and Cahenslyism. This work, despite weaknesses in presentation, should be of interest to students of ethnicity, immigration, and the Roman Catholic Church.

JOHN W. PRATT  
State University of New York,  
Stony Brook

HARRY A. KERSEY, JR. *Pelts, Plumes, and Hides: White Traders among the Seminole Indians, 1870-1930*. Gainesville: University Presses of Florida. 1976. Pp. xi. 158. \$7.50.

Correctly pointing out the need for a thorough ethnohistorical examination of the "remnant of a remnant," the Florida Indians who took refuge in the Everglades and Big Cypress Swamp after the Third Seminole War, Harry A. Kersey provides one segment of that overall story. He limits his analysis to "the impact of traders, missionaries, and government agents on the acculturational process of the Seminoles."

Taken in large part from narratives in the Florida Indian Oral History Archives, Kersey's sources are nevertheless exclusively non-Indian. They consist of reminiscences of surviving members of white trading families. Aware of this shortcoming, Kersey labels it a "historical tragedy" that Indians most involved in the boom years of exchanging alligator hides, otter pelts, and egret plumes for staples, left no narratives. He must depend, therefore, on impressions (albeit in most cases sympathetic ones) "imperfectly filtered through the perceptions of the traders." Apparently, field work has not retrieved anything resembling an Indian viewpoint on these years. Kersey is hopeful, however, that an upcoming generation of native American scholars conversant in the languages will be able to recover in some measure "their elders' experience."

Kersey details the symbiotic relationship which developed between each of the "permanent" traders and their Seminole customers. He emphasizes the trading post as an entrepôt in the two-way cultural exchange on the South Florida frontier. Indian resistance to Christianization and white-introduced educational institutions receives less attention.

The author's explanations for the trade's rapid decline after 1900 are convincing. Drainage of swamplands, encroaching settlement, overhunting, changing fashions and markets, and the conservation movement all contributed to the decline. Kersey also addresses the problem of whether the Seminole trade was excessively exploitative. He has carefully examined the extant business ledgers; he may be overly favorable to the traders. The

paucity of records and the absence of any Indian or even impartial white perspective preclude anything but tentative conclusions.

The publishers deserve a scolding for not including a map of South Florida.

MARTIN ZANGER  
The Newberry Library

RUTH Y. WETMORE. *First on the Land: The North Carolina Indians*. Winston-Salem, N.C.: John F. Blair. 1975. Pp. xvi, 196. \$8.95.

The fascination that Indian history and legend have for many people becomes apparent in the reading of Ruth Wetmore's close investigation of the aborigines of one state. Three linguistic groups lived in the four geographic areas of North Carolina: Algonquians on the coast, Siouans in the Piedmont, Iroquoians, including the Tuscarora, on the coastal plain, and the Cherokee, another Iroquoian group, in the mountains. Today only four states have a larger Indian population than North Carolina, and questions pertaining to Indian affairs are often before the people.

Ruth Wetmore's book is a carefully written synthesis of archeological reports, contemporary accounts of Indians by explorers and early settlers, and recent observation from her association with modern Indians. The first three chapters are historical, dealing with the land and the people in a general way, prehistory, the historic period, and the linguistic families; various wars, including those which involved the Tuscarora, Yamassee, and the Cherokee, are related; and in encyclopedic form twenty-eight different tribes are discussed. A chapter devoted to daily life deals with such topics as personal appearance, housing and settlement patterns, the economy, social organization, warfare, and games. Spirits, festivals, funerals, and medicine are discussed together, while a concluding chapter deals with the Indians of today. There are over 44,000 Indians now living in North Carolina, and they are found in more than twenty-six of the state's one hundred counties, ranging from a maximum of 26,486 in Robeson County (where they put the Ku Klux Klan to flight in 1958) to slightly more than a hundred in several others.

This book combines a very readable style, attractive and appropriate illustrations, maps, tables, and a bibliography to form a handy reference source on the subject. The current information on the Cherokee of the mountains and the Lumbee of the coastal plain, totaling over 34,000, is especially valuable, but accounts of other scattered groups, only recently formed into tribes, is also informative. A North Carolina Commission of Indian Affairs was established in 1971 to coordinate the use of federal funds to meet local Indian needs, and its



work with regional development associations and its own Neighborhood Youth Corps is described.

WILLIAM S. POWELL  
University of North Carolina,  
Chapel Hill

FRANK R. ROSSITER. *Charles Ives and His America*. New York: Liveright. 1975. Pp. xv, 420. \$15.00.

ROSALIE SANDRA PERRY. *Charles Ives and the American Mind*. Kent: Kent State University Press. 1974. Pp. xx, 137. \$7.50.

There seems to be much agreement among musical connoisseurs that Charles Ives was a composer of extraordinary talent. His contributions to music, especially the innovative aspects, are highly regarded, but the value of his music as part of the standard concert repertory is still debated. Ives chose the established insurance business world as the place to pursue his vocation. He composed music in a milieu largely of his own making. The business ventures brought him a fortune. The compositions brought him fame, however belatedly.

These two books provide generous amounts of carefully researched and well-written details of Charles Ives' life, his music, and his environment. Frank R. Rossiter's book, *Charles Ives and His America*, is a biographical study in a conventional format. Rosalie Sandra Perry's book, *Charles Ives and the American Mind*, is partially biographical but is primarily what she calls "a social psychology of music."

Rossiter's motif is that beneath "his self-effacing surface . . . Charles Ives was struggling both to maintain his masculinity in a female-dominated musical environment and to protect his creative independence in the face of popular disapproval of musical experimentation" (p. 45). There is a second and equally important theme: "Ives believed that in music, there was no virtue in merely following custom and authority; but it would never have occurred to him to apply the same philosophy to moral questions" (p. 169). The author develops his presentation in three parts—"Development (1874-1921)," "Recognition (1921-1974)," and "An Interpretation." Rossiter's book will appeal to both lay readers and scholars with musical experience. Perry's work, on the other hand, seems to be intended primarily for the latter group. The ability to read an advanced musical score is essential to a complete comprehension of the author's presentation. Thirty-four excellent examples, excerpts from Ives' musical scores, are included in the book. Some knowledge of late nineteenth- and early twentieth-century sociological and philosophical ideologies will also facilitate the reading of the book.

I believe that both books offer much to prospective readers interested in Charles Ives, his music, and the society in which he lived and worked.

ALBERT STOUTAMIRE  
McNeese State University

ODD SVERRE LOVOLL. *A Folk Epic: The Bygdelaag in America*. (Publications of the Norwegian-American Historical Association.) Boston: Twayne Publishers for the Association. 1975. Pp. 326. \$8.50.

*Bygdelaag*, in Norwegian, has no precise English equivalent. In Norwegian-American usage it denotes a society (*lag*) of emigrated persons from an old-country district (*bygd*). Usually rural, a *bygd* may or may not correspond to a political subdivision, but invariably it is associated with a particular folk culture, a culture claiming unique traditions and marked by a distinctive form of speech more or less comprehensible to other Norwegians.

Since *bygd* patriotism held a strong grip on most nineteenth-century Norse immigrants, the North American *bygdelaag* movement, which originated in 1899, attracted a large and enthusiastic following. It crested about 1914, by which time the founding of new *lags* had reached epidemic proportions, and annual *bygdelaag* rallies had become a highly visible feature of Norwegian-American social life. A promising but brief revival in the 1920s recovered some of the ground lost during World War I, but since the Great Depression there has been a general decline. It seems extremely improbable that the movement is profiting from the ethnic enthusiasm of the 1970s: *bygd* loyalty is a diminishing force in the consciousness of fourth- and fifth-generation Norwegian Americans.

In *A Folk Epic*, Odd S. Lovoll presents an excellent interpretive narrative of *bygdelaag* history. The book's most conspicuous virtue is the degree to which it responds to the needs of disparate audiences. *Bygdelaag* veterans and their progeny will appreciate the colorful anecdotes that enrich the narrative; and students of the American ethnic experience will profit from Lovoll's persuasive and balanced interpretation of the *bygdelaag* role within the context of Norwegian, American, and Norwegian-American history, a context which the author has mastered very effectively. Nor is this achieved at the expense of unity and coherence, for the anecdotes, though appealing in their own right, strengthen the interpretive dimension.

Laying the groundwork for *A Folk Epic* involved activity which per se is a contribution to historical scholarship. For many years *bygdelaag* leaders working in cooperation with the Norwegian-American Historical Association collected yearbooks and a variety of other sources relating to *bygdelaag* history, a process completed by Lovoll when he undertook the project. This material, which covers most fac-



ets of Norwegian-American history, is now held by the NAHA archives at St. Olaf College and is accessible to scholars.

CARL H. CHRISLOCK  
*Augsburg College*

ROBIN HIGHAM, editor. *Intervention or Abstinence: The Dilemma of American Foreign Policy*. Lexington: University Press of Kentucky. 1975. Pp. 221. \$14.75.

This collection covers a series of twentieth-century crises in which the United States intervened. In a lengthy introduction Robin Higham traces American expansion back to 1783. Kenneth J. Hagen then asserts that since the 1870s, the navy has uncritically seen itself as the guardian of national property and promoter of overseas commerce.

Moving to Latin America, P. Edward Haley compares Wilson's intervention in Veracruz with Lyndon Johnson's dispatch of over 25,000 American troops to the Dominican Republic. In both cases the United States intervened in order to stop revolutionary violence, establish an interim coalition friendly to the United States, hold free elections, and protect foreign investment and local private enterprise. James C. Carey presents a thorough and revealing description of the holdings of the United States in Chile and Peru and shows that by 1950 the nation had used up the trust fostered by the Good Neighbor policy.

Three essays focus upon the Middle East and the Eastern Mediterranean. Dennis Deutsch briefly surveys Zionist attempts to gain American recognition for Israeli statehood. Such special-interest politics, Deutsch argues, can jeopardize "whatever democratic value is left in our political system" (p. 90). Janice J. Terry discusses Dulles' cancellation of the Aswan Dam project. Although the secretary perceived the Egyptians as instruments of Moscow, the very way in which he handled the project guaranteed the 1956 war and the short-term decline of Western influence. Theodore A. Coulombis and M'Kean M. Tredway note how long the United States has intervened in Greece's internal affairs without ever defining interests and priorities.

The rest of the essays deal with Asian embroilments. Norman A. Graebner's treatment of the Manchurian incident offers a trenchant critique of the Hoover administration. By committing the United States to the status quo, it ignored its own interests and faced eventual war with Japan. William L. Richter's account of the Bangladesh crisis of 1971 indicates that America acted ineffectively and upon doubtful premises. Ted Goertzel and Donald J. Mrozek contribute essays on Vietnam, the former noting the initial moderation of war opponents and the latter focusing upon the use of surrogate forces and air strategy.

Alas, every aspect of this volume bears the impression of haste. Unclear allusions, uncompleted quotation marks, misplaced commas and dashes, long and convoluted sentences, authorities mentioned in the text but uncited in the bibliography—all could serve as negative models for a writing seminar. Treatments of Mexico, Korea, and Vietnam offer little not found in a good textbook. Certain claims remain unproven, such as Deutsch's stress on Eddie Jacobson's role in Truman's Palestine policy and Goertzel's assertion that American intervention in Vietnam was rooted in "the ideologies and class interests" of the policy makers (p. 171). More skillful editing could have prevented this volume, which contains several useful essays, from becoming an uneven series of articles in search of an anthology.

JUSTUS D. DOENECKE  
*University of South Florida,  
New College*

AUBREY PARKMAN. *David Jayne Hill and the Problem of World Peace*. Lewisburg: Bucknell University Press. 1975. Pp. 293. \$15.00.

It is difficult to write a meaningful and exciting biography about a somewhat lackluster subject, especially when, as in this case, the person was of less than first-rate importance. Aubrey Parkman's biography of David Jayne Hill labors with mixed results to overcome these handicaps. Hill had three almost distinct careers: educator, diplomat, and political publicist. Each was interesting in its own right, but the book gives us too little sense of how they related to each other or how they formed the total figure, public and private.

Hill's academic success came exceptionally early. He graduated from Lewisburg College (later Bucknell) in 1874, was appointed an instructor the following year, and was named president in 1879 at the age of 29. He was able to modernize the school's curriculum and increase its endowments while still finding time to teach and write textbooks on logic, rhetoric, and psychology. He was so successful, in fact, that the University of Rochester hired him as president in 1888. There he performed similar feats, and a notable future in higher education seemed in order. But his effort to de-emphasize the university's Baptist sectarianism led to friction with the trustees, and in 1896 he resigned. Parkman has little more to say about this period of Hill's life and the impact it had on his later experiences.

Courting Republican officials by stump speaking and writing campaign literature, Hill managed to gain an appointment in 1898 as assistant secretary of state. Loyal and industrious, he never was in the forefront of events. Parkman demonstrates, however, that Hill helped resolve difficult issues at

the Pan-American Conference of 1901, that he was an effective delegate to the Second Hague Peace Conference, and that, as minister to Switzerland and ambassador to Germany, he performed creditably. He was anything but indispensable, however, and in 1911 Taft recalled him from Berlin in favor of a wealthy contributor to the GOP.

After returning to the United States, Hill, for the last twenty years of his life, wrote a torrent of political tracts and a few serious works on diplomacy. Parkman's book is strongest in this section, for he subjects Hill's ideas to sharp analysis, exposing contradictions in his thinking on both foreign and domestic affairs. A long-time advocate of international law and arbitration, Hill denounced the League of Nations as a sinister plot hatched by Wilson. Even the World Court was a dangerous body to join, since it might lead to backdoor entry into the League. Hill said that peace must be "organized," but voluntarily and with no impairment of national sovereignty, especially United States hegemony. It is hard to dispute Parkman's conclusion that Hill had little to offer toward an understanding of the era's global problems. He might better have remained a college president or diplomat.

Despite its weaknesses in integration and character development, the book is carefully researched and clearly written, and it should be useful to diplomatic specialists of the period.

RALPH STONE  
Sangamon State University

JEFFERY M. DORWART. *The Pigtail War: American Involvement in the Sino-Japanese War of 1894-1895*. Amherst: University of Massachusetts Press. 1975. Pp. 168. \$9.00.

This is the story of Secretary of State Walter Q. Gresham's difficulties during the first Sino-Japanese War of 1894-95. Seekers of fame and fortune ignored Gresham's efforts to preserve neutrality, but the secretary denied protection to these adventurers. The tendering of the good offices of the United States also opened the door to difficulties. In China and Japan mobs attacked subjects of the hated enemy, and efforts of American officials to protect them led to ill will. The large number of Japanese spies in China who sought protection further complicated the situation. The American consul of Shanghai, intent on preserving the sanctity of the foreign settlement, sought to shield these Japanese. Gresham held firm against such actions in the face of the consul's pleas that the Japanese culprits would suffer torture.

American missionaries in China, jittery in the face of danger, pleaded for protection. Minister Denby in Peking called for troops. Gresham and

President Grover Cleveland, although sympathetic to the missionaries, opposed sending marines. Finally they gave their approval, and a contingent of marines did reach Tientsin. American naval ships patrolled the waters near the scene of the fighting, and small gunboats went up the Yangtze as a warning against antimissionary outbreaks.

Contrary to the intentions of Cleveland and Gresham, Minister Denby and the former secretary of state, John W. Foster, went far beyond the role of serving as messengers in the exchange of views between the belligerents, and they became advisers to the Chinese government in the peace negotiations.

The author presents a well-balanced and tightly written account. He believes that the war revived American interest in Asia and set precedents for the future. He concludes that his documentary study illustrates "how an idealistic administration, pressed by a critical public and caught by its own conceptions of moral and legal duties, can entangle the United States just as deeply in a foreign war as any expansionist government guided by motives of power and self-interest." This broad generalization goes well beyond the evidence. In spite of the fact that the book deals with a minor episode in American foreign relations, it is a worthwhile contribution.

PAUL A. VARG  
Michigan State University

DAVID HEALY. *Gunboat Diplomacy in the Wilson Era: The U.S. Navy in Haiti, 1915-1916*. Madison: University of Wisconsin Press. 1976. Pp. ix, 268. \$15.00.

This book is an elegantly written narrative and evaluation of the Navy's role in the critical first year of the Haitian occupation of 1915. In the initial months of what became a nineteen-year imperial experiment, the United States executed its Haitian policy through naval and marine officers of the cruiser squadron, a force assigned to patrolling the Caribbean during these years in order to safeguard the Panama Canal and to protect foreign lives and property in revolutionary disturbances.

In the half-dozen years preceding this intervention, Haiti suffered the political and economic maladies familiar to other Caribbean republics. Its distinguishing characteristic was its status as a black republic dominated by a French-speaking elite. Like their Spanish-speaking counterparts in the Dominican Republic (occupied by American forces in mid-1916), members of the Haitian elite ruled in their own interests with little regard and even disdain for the common people. For their salvation Woodrow Wilson offered tutelage in the art of republican government and civic morality.

Despite the primacy of Caribbean policy since 1898, neither the State nor Navy department had adequately prepared the Navy for this crucial role it played in Haitian affairs. The central figure in this work is, thus, not Wilson or Robert Lansing, but Admiral William Banks Caperton, a somewhat old-fashioned career officer with no training in diplomacy and little knowledge of Haitian history. Following the bloody mob killing of Haitian executive Vilburn Guillaume Sam (retold here in gruesome detail), Caperton became the authority in the republic. He restored order, convinced other foreigners they would be protected, denied the presidency to the fiery nationalist Dr. Rosalvo Bobo, installed the compliant Sudre Dartiguenave in the executive mansion, dispensed food to starving Haitians, and, by a combination of tact, intimidation, and bribery, negotiated a Haitian-American treaty allowing the United States broad political and economic supervision over Haitian affairs. When *caco* rebels threatened American rule, they were suppressed, brutally but effectively by marines. When the Haitian legislature resisted efforts to modify the constitution to accommodate alterations in the Haitian-American relationship and threatened to impeach the president, Dartiguenave, with Caperton's reluctant backing, disbanded it. As Healy points out, such arbitrariness, necessitated perhaps by the course of events, ultimately violated the American objective of instilling respect for constitutional government in the tropics.

Healy is not uncritical of American policy toward Haiti, but he makes no sweeping judgments about economic imperialism or racism. Concerning the latter, he explains that the prevalent racial attitudes doubtless influenced American thinking, but he notes that Caperton had comparatively mild racial beliefs and that the admiral's French-speaking assistant, Captain Edward L. Beach, exhibited notable respect for Haitian culture. In contrast to another recent work on this topic (Hans Schmidt, *The United States Occupation of Haiti, 1915-1934* [1971]), Healy argues that it was not planning in Washington that determined the course of this occupation, but problems encountered on the scene.

LESTER D. LANGLEY  
University of Georgia

camp." In this first full-scale history of that tiny island in New York harbor, Thomas M. Pitkin shows why nobody was neutral about it. Perhaps twelve million aliens, beginning with Annie Moore of County Cork on January 1, 1892, passed through the United States immigration station on Ellis Island, which also served as a major deportation center until it was vacated in 1955.

Pitkin necessarily focuses on administration. He records such details as the exact dimensions of the immigrant reception building, which replaced the wooden structure destroyed by fire in 1897, yet he is chiefly concerned with people and decisions affecting their lives. After sketching the island's earlier history, he describes the gigantic administrative problems of an immigrant reception center coping with more arrivals than it could handle, interning enemy aliens during World War I, adapting to the hysterical postwar pursuit of radicals and criminals, complying with the quota laws of the 1920s and, particularly after 1930, the policies of detention and deportation of aliens. The author concludes with Ellis Island's transformation into a national monument to be associated with the immigration museum at the base of the Statue of Liberty. It is unfortunate that Congress has not appropriated the necessary funds.

To be commissioner of immigration in New York was to hold an unenviable job: to execute the law with inadequate resources and in the face of pressures from shipping interests, politicians, restrictionists, antirestrictionists, and civil libertarians. Pitkin neither whitewashes nor condemns federal policies. He sympathizes with most of the commissioners, notably William Williams, a conscientious restrictionist; Robert Watchorn, an advocate of free immigration; and the reformer Frederic C. Howe, who resigned under fire in 1919 for bucking the policy of wholesale deportation of alien radicals.

The book lacks an appendix. To find no list of New York commissioners with their periods in office, and no statistical tables of immigrants or deportees, is a disappointment. Nevertheless, Pitkin's extensively documented work will be essential for anyone seriously interested in American immigration history.

ROBERT ERNST  
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THOMAS MONROE PITKIN. *Keepers of the Gate: A History of Ellis Island*. New York: New York University Press, 1975. Pp. xiii, 226. \$12.50.

Ellis Island has been called many things, including "the Plymouth Rock of its day," "Island of Tears," and "America's first concentration

ALEXANDER SAXTON. *The Indispensable Enemy: Labor and the Anti-Chinese Movement in California*. Berkeley and Los Angeles: University of California Press, 1975. Pp. x, 293. \$3.95.

The literature on racism and its influence on California history is voluminous. Much of it is, unfor-

unately, ambitious in conception, but disappointing in execution. What frequently emerges from such efforts is a reiteration of old findings sometimes garbed in new prose. There are exceptions. Roger Daniels, Gunther Barth, Carey McWilliams, and Elmer Sandmeyer made individual contributions, and all serious students come to their work early in any effort to deal with this topic. Now there is a new contribution. Hereafter, Alexander Saxton's *The Indispensable Enemy* will provide an essential source for information and insight into this subject. Like others of this genre, it is a serious work that deals intensively with the themes suggested in the title. It provides not only the broad overview, but a stimulating study of the subtleties, complexities, and revealing nuances of the interrelationships between the anti-Chinese movement and California's organized labor movement.

The book is based upon an extensive review of the materials available, although the lack of a bibliography makes the determination of that fact unnecessarily difficult. One might question the absence of an item or two from either the footnotes or the bibliographic note. A more serious objection is a reflection of the background of the author: he used no Chinese-language sources. There are, no doubt, difficulties in obtaining such materials, and mastering the language is a formidable barrier for most historians. Nevertheless, a serious scholarly study of the place and role of an ethnic group in American history cannot safely ignore the materials produced by that group.

The examination of the non-Chinese language materials was extensive and, more importantly, the materials were used very well. As a consequence we learn something about the pervasive nature of anti-Chinese sentiment and the manner in which it warped the thinking of both rank and file, as well as that of devoted and sophisticated labor figures. Unable to withstand the appeal of prejudice, sometimes infused with bigotry, sometimes finding it expedient to pander to the prejudices of those they aspired to lead, sometimes isolated from their followers when they stood against the tide, such men became part of a tragic episode of labor history. The labor movement became further fragmented, and its solidarity, which was a source of great potential strength, dissipated. Locked in a deadly embrace, the persecuted and the persecutors were victimized. In good times and bad, the purveyors of racist notions diverted the attention of the members of the organized labor movement from real problems. And the lowly Chinese, the most exploited segment of the laboring class, were perceived through racist prisms as the problem and the enemy. The review of this interaction might well be studied by those segments of the present population who share sim-

ilar problems, but who are inclined to find responsibility for their plight in other victims.

DAVID A. WILLIAMS  
California State University,  
Long Beach

ALPHONSO PINKNEY. *Red, Black, and Green: Black Nationalism in the United States*. New York: Cambridge University Press. 1976. Pp. xii, 258. \$13.95.

Alphonso Pinkney has set for himself a two-fold purpose in *Red, Black, and Green: Black Nationalism in the United States*. Defining "the elements of black nationalist ideology" and focusing on such historically significant black nationalists as Paul Cuffe, Martin R. Delany, Bishop Henry M. Turner, and Marcus Garvey, Pinkney has, first of all, written a general history of black nationalism in America from the late eighteenth century to 1963. He has then devoted the remaining three-fourths of his book to a study of the re-emergence of black nationalism from 1963 to 1973. It is to these latter years that Pinkney ascribes primary importance, and it is the thesis of his book that "the ascendancy of nationalist ideology in the black community in the past decade has resulted in greater unity among Afro-Americans than existed in any previous period of history, and that black liberation, however defined, depends on such unity." Pinkney's probing of contemporary black nationalism is both comprehensive and provocative, especially his contrasting of its cultural and revolutionary dimensions; and any student of recent black turmoil and aspirations will find this volume an essential secondary source. Evidently a black nationalist himself, Pinkney notes with disappointment that in 1975 black nationalism is again "in a state of flux and apparent decline."

Although Pinkney is a sociologist, he is also a competent historical synthesizer. At the same time, his description and analysis of the forerunners of contemporary black nationalism add next to nothing to what scholars have already learned from the writings of E. David Cronon, Harold Cruse, August Meier, Edwin S. Redkey, Elliott Rudwick, Theodore Vincent, and others. Moreover, Pinkney acknowledges that "omissions abound" in his historical survey of black nationalism. This is understandable, given the scope and complexity of his subject. But some of these omissions are too obvious to ignore—for example, the nationalistic impetus behind the establishment of all-black towns and the nationalistic implications of Booker T. Washington's programs for black economic and educational development. And Pinkney should have explored more diligently and insightfully two questions that have persistently confronted the student of black nationalism: Why

did the nationalistic aspect of the "two-ness" of Afro-American ideology emerge dramatically at some times and not at others? And to what social classes in black America did nationalistic ideology offer a compelling program of self-affirmation and action, and why?

WILLIAM M. TUTTLE, JR.  
*University of Kansas*

NORRIS HUNDLEY, JR., editor. *The Chicano*. Introduction by MATT MEIER and FELICIANO RIVERA. Foreword by MIGUEL LEÓN-PORTILLA. (Clio Books/Pacific Historical Review Series.) Santa Barbara: Clio Books. 1975. Pp. xi, 1968. Cloth \$9.50, paper \$4.75.

The overall weakness of this volume is its lack of a theme more specific than the history of the Mexican-American in the United States. To be sure, that is a theme, but the volume demonstrates that it is much too broad to provide cohesion. The strength of the book is that several essays are significant contributions, and one is a major contribution to the literature.

The volume, which was originally a topical issue of the *Pacific Historical Review*, begins logically enough with a bibliographic and quasi-theoretical overview by Arthur Corwin of the Mexican-American historical experience. It has major shortcomings. As a bibliographic essay it is incomplete, and as a theoretical effort it makes no attempt to improve on the rather crude periodizations of Rodolfo Álvarez and others. Rodolfo Acuña in the next essay notes Corwin's other shortcomings. This is followed by Carey McWilliams' chatty, interesting, informative, and probably inconsequential essay on the background to writing *North from Mexico*.

The Texas experience is represented by Félix D. Almaraz's essay on the early years of that most untypical Mexican-American, Carlos E. Castañeda. The essay by William B. Taylor and Elliott West is the best in the volume, and it is a major contribution. What little we know about the making of a *patrón* and the nature of the system is greatly expanded by this essay. Charles Wollenberg's essay on the 1903 Pacific Electric Railway strike is a welcome contribution to the literature, as is the essay on the Mexican experience in Gary, Indiana, during the Depression. Abraham Hoffman's essay on the origins of repatriation in the same period is treated more fully in his book. The volume closes with Richard Nostrand's informative essay on the terms Mexican-Americans use to describe themselves, that is, Chicano, Hispano, and so on.

The volume's major defect, aside from its unevenness, is its neglect of the Texas experience and its complete silence on Arizona and New Mexico. Robert Larson's recent essay in the *Pacific Historical*

*Review* on white capping in San Miguel County would have been an excellent addition to the work.

G. L. SELIGMANN, JR.  
*North Texas State University*

WILLIAM L. BOWERS. *The Country Life Movement in America, 1900-1920*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1974. Pp. 189. \$10.00.

One of the more appealing ways to look at periods or movements in American history is to see them as mixtures of old and new, blending elements of both the past and future. Indeed this point of view can become a meaningless generalization because it may be (and has been) said of almost every era in American life. Yet despite its overuse, it does offer a convenient perspective for analyzing events, ideas, and social changes.

Thus William L. Bowers interprets the country-life movement in the first two decades of the twentieth century as a heroic, but ultimately futile, effort to preserve the ideals and values of rural society in a complex urbanized world. Combining "rural nostalgia, the desire to make agriculture more efficient and profitable, humanitarianism, and economic self-interest," the movement, in Bower's opinion, did not want to return to a primitive agricultural economy; rather, it wished to defend the social and political virtues of the agrarian past while retaining the material benefits of the industrial present. Living in a transitional period, the movement's leaders sought for the farmer the best of both worlds: the individualism and self-reliance of the nineteenth century as well as the technological and organizational achievements of the twentieth.

From this vantage point the country-life movement may be seen as a model for the entire Progressive impulse. Bowers is quick to emphasize the similarities: the leadership of the movement was relatively young, middle-class, urban, Protestant, and well-educated; its members believed in science, education, and expertise as instruments to improve rural life; its proposals envisioned more governmental assistance at the same time that they glorified the yeoman myth; its rhetoric described agriculture both as a business in need of better management and as a morally superior way of life.

These conflicting notions, according to Bowers, resulted in the eventual failure of the movement itself. The champions of rural reform did not arrest the migration from farm to city, nor did they prevent the urbanization of those who remained on the land. They neither regenerated the rural church nor alleviated the wretched conditions of tenancy and sharecropping. Above all, Bowers contends, the leaders of the movement had no real



rapport with ordinary farmers, which was just as well since they also failed to recognize that the thrust of industrialism would be too strong to overcome.

Bower's interpretation is concise, provocative, and persuasive. His argument is important, especially its implications for an understanding of Progressivism as a whole. Despite his reliance on a generalization rapidly becoming a cliché, Bowers has written an extremely valuable book.

RICHARD H. PELLIS  
*University of Texas*

LUCY LILIAN NOTESTEIN. *Wooster of the Middle West*. Volume 2, 1911-1944. Kent: Kent State University Press. N.d. Pp. xi, 433.

ALLAN G. BOGUE and ROBERT TAYLOR, editors. *The University of Wisconsin: One Hundred and Twenty-five Years*. Madison: University of Wisconsin Press. 1975. Pp. x, 289. \$7.50.

The two Middle Western schools described in these histories suggest the divergence of types among American institutions of higher education. Wooster College (formerly University) originated as a denominational enterprise to provide Ohio Presbyterians with higher learning "on Christian principles." Among church-connected colleges, however, Wooster was unique in long continuing to send many students into ministerial careers, especially in the foreign mission field. Wooster had a professor of missions beginning in 1903. Conversely, missionaries sent their children to Wooster, giving it an unusually cosmopolitan flavor. The University of Wisconsin symbolizes the public institution of large size and broad vision, responsive to more publics than had claims upon the private college. Among state and federal land-grant foundations, however, Wisconsin had its own singularity—the "Wisconsin Idea" of making the "beneficent influences of the University available to every home in the state."

The account of Wooster College's early years was published in 1937 and reissued in 1971 as a companion to the present volume; this second volume redeems Lucy Notestein's promise to continue her chronicle "in due time" and after sufficient years had elapsed to confer perspective. She brings to institutional history the insider's advantage of being the daughter of a central faculty figure, the alumna's devotion to gathering and reporting myriad small facts, and the human-interest approach. There are intrinsically important events in Wooster's history and instructive case-study materials for historians of American higher education that are located amid the trivialities of the cost-per-thousand of construction bricks, the outcomes of forgettable athletic contests, and the

idiosyncrasies of professorial personages. President Holden's manipulation of Henry Clay Frick is enlightening given the continuing saga of philanthropy's encounters with academe. Wooster's struggles with the fraternity system represent a general issue in higher education and a particularistic perception of an un-Christian caste system in a Christian college. The tensions between liberal education and vocational preparation, most persistently focused at Wooster on teacher education, were echoed elsewhere. The pressures of professionalism, scientism, athleticism, and even secularism, and their results in institutional imitation, which made Wooster more like Wisconsin in 1944 than it had been in 1910, surface in this loving but not entirely uncritical history.

The elements that reclaim Notestein's book hardly mark the collection of essays on the University of Wisconsin, the largest part concerning its past quarter century. To place the university's recent history in the context of its whole development, a thirty-five-page distillation was made of *The University of Wisconsin: A History, 1848-1925* (1949) by Merle Curti and Vernon Carstensen. But the qualities that unquestionably excepted their work from the usual characterization of institutional historiography as hagiography segmented by presidential administrations are not easily preserved by condensation. Nevertheless, this chapter remains the most generally interesting and solidly historical part of the collection. Then follows Mark Ingraham's survey of institutional events in the period from 1925 to 1950. He recounts organizational and curricular elaborations, describes and evaluates, but does not explain, such short-lived phenomena as the regents' refusal to accept foundation gifts, and scatters clues about the introduction and repudiation of the experimental college of Alexander Meiklejohn and President Glenn Frank, "a segregated non-coeducational, imported program, administratively sponsored and approved only as an experiment by the faculty, [and which] should not have been expected to survive." Chapters follow on the physical plant, finances, faculty governance, student "importance" (activity), and curricular change; there are essays on research and new courses in the several sciences, humanities, and arts, but nothing on the professional schools. A concluding chapter describes the "outreach instrumentalities and arrangements" by which the Wisconsin Idea is today expressed. An account of the Wisconsin state system of higher education by Clara Penneman is more interpretive than its fellows.

To those seeking provocative institutional sagas, embedded in social and intellectual history, Wisconsin offers two recent dissertations: William R.

Johnson's history of the university's law school and Dagmar Schultz's of the Wisconsin school for workers. The university's official history, however, disappoints. Its authors, key figures in the limited internal and organizational history that they describe, write a faculty-oriented narrative which reads like academic reports seasoned with autobiographical musings. Insatiable academic politicians elsewhere and present or former Wisconsin faculty are more likely to appreciate this book than are alumni, who will find it dull and smaller than was life at their university.

GERALDINE JONCICH CLIFFORD  
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GLEN O. ROBINSON. *The Forest Service: A Study in Public Land Management*. Baltimore: Johns Hopkins University Press, for the Resources for the Future. 1975. Pp. xv, 337. Cloth \$16.95, paper \$4.95.

Glen O. Robinson presents a broad view of the role of the United States Forest Service in public land management. The work is carefully planned. Chapters on the history of the Forest Service and on Forest Service bureaucracy provide the background for the chapters that follow: resources and uses; timber resource; outdoor recreation; wilderness; range resource; wildlife; and water and the watershed. He closes the discussion with "The Forest Service in Retrospect," a very interesting summary.

There have been three approaches to the management of public forest lands: intensive management catering to commercial interests; preservation or returning lands to their primitive state to satisfy those interested in recreational use; or management by the professional land managers, in this case, the United States Forest Service. The proponents of the various approaches are constantly reacting as the pressures on the limited land base increase.

The Forest Service as the administrator of the national forests is frequently in the middle. It has been charged that its traditions of professionalism have resulted in the service's following a conservative policy. Moreover, the public has not, some have felt, been included in the planning and decision-making process. Robinson concludes that "what is wanted is a somewhat greater sensitivity to public sensibilities, even those that are based on misdirected anxieties."

His is a stimulating approach to the study, in that he presents the various views and their background, implications, and effects. Though biases are at times apparent, Robinson's treatment reflects his legal background and the need to discuss more than one point of view.

His work is carefully footnoted with factual

notes at the end of each chapter, reflecting the scope of material consulted. There is, however, no bibliography. His appendixes include, by region and state, national forest areas including game refuges, recreation, wilderness, and primitive areas, and grass lands.

Historians, environmentalists, various government officials, and others will find this book of value. Robinson's interweaving of the historical background and his insight into the problems of conflicting interests in the use of resources within the lands administered by the United States Forest Service make this a valuable contribution to the literature on public land management.

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ARI HOOGENBOOM and OLIVE HOOGENBOOM. *A History of the ICC: From Panacea to Palliative*. New York: W. W. Norton and Company. 1976. Pp. xi, 207. Cloth \$10.00, paper \$2.95.

Despite changes in historical fashions, the history of transportation continues to appeal to a large group of scholars. Most studies in this field deal with specific topics, limited in breadth, time, and place. Hence this slim volume is a welcome addition to the literature, since it surveys the course of railroad and ICC regulation over the span of the past century.

Ari and Olive Hoogenboom have divided their work into five succinct chapters. In surveying the background of the Interstate Commerce Act of 1887 they skillfully dissect the diversity of interests that pressed for government control. Chapter 2 provides a mature analysis of the ICC in the progressive era, when shippers dominated transportation policy to the detriment of the railroads. Chapter 3 delves into World War I and the 1920s. These were years of lost opportunity as shortsighted railway and labor executives and timid ICC commissioners failed to formulate and implement plans for a rational railroad and transportation system. Chapter 4 focuses on the Great Depression and World War II, which left the railroads in an increasingly weakened and helpless condition. The final chapter deals with drifting and static ICC policies in the three decades after World War II. The Hoogenbooms conclude that the lack of a clear and well-defined transportation policy in 1975 is the result of a century of drift and indecision, due increasingly to an entrenched and sterile ICC bureaucracy.

By its nature, this is a synthetic work rather than a product of original research. It summarizes significant works in the field, including studies by Benson, Carson, Friendly, Friedlaender, Kirkland, Kolko, Martin, and others. Specialists will find

little that is new in it. But *A History of the ICC* is designed for laymen and students, and the authors have succeeded in introducing them to the complexities of the subject.

GERALD D. NASH  
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FREDERICK A. BODE. *Protestantism and the New South: North Carolina Baptists and Methodists in Political Crisis, 1894-1903*. Charlottesville: University Press of Virginia. 1976. Pp. viii, 171. \$9.75.

This study delineates political involvements of the two largest white religious bodies in North Carolina at a time when a fusion of Populists and Republicans vied with Democrats for ascendancy in the state. Though Tarheel churchmen had not ignored secular developments before, their alignments in this decade were extraordinary by past standards. Frederick A. Bode describes patterns somewhat akin to those reported earlier in Liston Pope's more localized *Millhands and Preachers: A Study of Gastonia*. Both authors tell of compatibilities between Protestant churches and an elitist temporal power structure.

Bode emphasizes the churches' interests in religious colleges and other denominational enterprises—in fund-raising, in black subordination, in improving common school education, and in promoting a more prosperous and industrialized economy. He also underscores the ecclesiastical leadership's vigorous advocacy of these concerns. Sectarian opposition to appropriations for the state university became intertwined with a crusade to build a Democratic hegemony that New South architects could dominate. Compared to religious hierarchies in other Southern states, Baptist and Methodist spokesmen in North Carolina seemed notable from 1894 to 1903 for the extent to which they embraced essentially political causes; their contributions to a renewal of white solidarity were considerable. This book serves as a further corrective of the untenable view that spiritual labors fully absorbed the energies of Southern clergymen before World War I; it also undermines a contention aired by Walter Hines Page and others that Protestantism was a foremost barrier to Southern advancement in that era.

Readers will appreciate the author's attention to organization, his easy-to-follow style, his perception of the subtleties of coalition politics, his evenhanded assessments, and his extensive use of both manuscript and printed materials. The volume provides much that is fresh and significant. But several unfortunate omissions will be noted. Strangely unacknowledged, for example, are the two best-known general scholarly works on Southern Baptists and Methodists in the late nineteenth

century—Hunter D. Farish's *Circuit Rider Dis-mounts: A Social History of Southern Methodism, 1865-1900* and Rufus B. Spain's *At Ease in Zion: Social History of Southern Baptists, 1865-1900*. On one point relating to Southern religion, Bode cites Robert Ellis Thompson's eighty-year old study, *History of the Presbyterian Churches in the United States*, but Ernest Trice Thompson's recent and more creditable three-volume set, *Presbyterians in the South*, is altogether ignored.

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El Paso*

BLAINE A. BROWNELL. *The Urban Ethos in the South, 1920-1930*. Baton Rouge: Louisiana State University Press. 1976. Pp. xxi, 238. \$12.50.

This is a book about Southern urban boosterism. It covers Atlanta, Birmingham, Charleston, Knoxville, Memphis, Nashville, and New Orleans in the 1920s. Relying upon printed primary sources such as newspapers and chambers of commerce publications, Blaine A. Brownell presents ample evidence to support his thesis that civic leaders had a "conception of the city, a guiding complex of beliefs concerning the nature and role of the urban community—an *urban ethos*—[which] was maintained and expressed by spokesmen of the leading white commercial and civic groups" through a variety of media. Briefly, the urban ethos was a commitment to growth. When growth brought social fragmentation, promoters hoped to impose unity by emphasizing "common goals," particularly "growth and progress."

There are no surprises in this book. Not many students of United States history embrace the old notion that the South was antiurban. Few historians will raise their eyebrows at Brownell's discovery that boosters had little social conscience. His contribution lies in taking the ideas of others and applying them to the South. This book reflects a major intellectual debt (which he acknowledges in the notes and bibliography) to A. Theodore Brown, Charles N. Glaab, R. Richard Wohl, and Anselm Strauss.

Teachers of urban and social history will find *The Urban Ethos* useful. Particularly attractive are the illustrative quotations from previously neglected booster literature. The most distracting dimension of the book is its style. One wonders if Louisiana State University Press employs an editor. The phrase "commercial-civic elite" is used ad nauseam. I counted it twenty times in chapter 6. Equally frustrating are the ambiguities. Page xviii is typical: "It is impossible to know precisely how much the circulating media influenced the opinions of the city populace, but it is probably accu-

rate to say that the majority of printed materials both shaped and reflected public opinion, in varying degrees." Problems such as these limit the appeal of this book.

LYLE W. DORSETT  
University of Denver

JAMES T. PATTERSON, editor. *Paths to the Present: Interpretive Essays on American History since 1930*. Minneapolis, Minn.: Burgess Publishing Company. 1975. Pp. vii, 230, \$4.95.

In recent years numerous edited works, often compiled hastily and without original contribution, have flooded the textbook market. Fortunately, *Paths to the Present* does not fall into this category, although the title is certainly much too familiar. The five original, interpretive essays, all of which analyze American developments since 1930, overlap surprisingly little, and each essay nicely complements the other. All stress, or at least mention, the enormous change produced by the Second World War; all assess both continuity and change; and all (to varying degrees) are cautiously optimistic about the future, despite the divisive events of the recent past. Moreover, the essays cannot be easily categorized either as new left or traditional liberal, and the collective result represents a persuasive analysis of recent major trends.

Editor James T. Patterson chose his contributors wisely. In the lead essay, William H. Chafe accepts the difficult task of analyzing the extent of social change since 1930 and focuses on the shifting status of women, which he considers a "barometer of how American society operates." Patterson discusses American politics and the two great reform movements since 1930, emphasizing that the "patchwork" condition of the welfare state of the 1970s resulted less from weaknesses in planning or execution than from the formidable barriers to sweeping innovation. John Garry Clifford emphasizes change and continuity in American foreign policy, stressing the impact of the Second World War in producing profound alterations and suggesting that the Vietnam War may have been "both the logical culmination of American foreign policy since 1945 and a turning point comparable to that of World War II." Richard O. Davies examines the city in modern America, concluding that its heritage is ambivalent and its future uncertain. Finally, Harvard Sitkoff in a well-balanced essay on race relations, which could have been improved had he expanded his assessment to include racial minorities other than blacks, contrasts the deplorable condition of Negroes in 1930 with the much brighter prospects of the 1970s. In short, students and scholars alike will enjoy and profit

from these syntheses, which represent the best in historical scholarship.

RICHARD T. RUETTEN  
San Diego State University

THEODORE ROSENOF. *Dogma, Depression, and the New Deal: The Debate of Political Leaders Over Economic Recovery*. Port Washington, N.Y.: Kennikat Press. 1975. Pp. xiii, 155. \$12.50.

Historical debate about measures to overcome the Depression of the 1930s is almost as old as the New Deal itself. Theodore Rosenof's contribution to this discussion eschews the more common approach that focuses upon events, programs, and administration disputes, in an attempt to discover the fundamental ideological issues at stake. He examines the writings and speeches of leading spokesmen for the New Deal and their conservative and radical critics to discover underlying commitments and biases.

The key to the New Deal analysis of the Depression, he contends, was its "purchasing power thesis," or the belief that stagnation and decline were caused by low wages and underconsumption. The story of efforts to compensate for lost purchasing power in programs such as the NIRA and AAA are familiar to any student of the depression decade. Where the author's contribution lies, however, is in the careful delineation of the various positions taken by important groups on these and subsidiary issues like the causes of the Depression, the sources of the 1920s prosperity, and the historical role of the frontier. Most significant is his argument that planning was the most controversial of all issues, even to New Deal stalwarts. The New Deal, he concludes, was neither prepared nor willing to experiment sufficiently to break down the ideological barriers to a true "inclusive pragmatism." Inevitable, cures for the Depression were "dogmatically limited."

Much of the material here assembled is worthwhile and the author's treatment of it is interesting. The book might have been improved, however, by closer attention to style and editing.

JAMES B. GILBERT  
University of Maryland,  
College Park

JUDITH ANN TROLANDER. *Settlement Houses and the Great Depression*. Detroit: Wayne State University Press. 1975. Pp. 216. \$12.95.

Judith Trolander in this book argues that the primary determinant of the political response of settlement house workers to the New Deal was the type of funding that supported the settlement. Those funded from a central community chest



source ostensibly "ignored social issues"; those supported by their own boards, namely settlement houses in New York and Chicago, were more committed to "social action programs." It is unfortunate that the evidence presented on behalf of the hypothesis that "the decisive factor" was "the presence or absence of a Community Chest in any given city" is simply too limited (p. 10). Trolander has researched energetically in archival materials, but she has trapped herself in an excessively restricted framework and must, therefore, modify and contradict her formulation time and again.

Let me pinpoint some of the weaknesses, with information and contentions usually drawn from Trolander herself. Settlement house workers were not in the forefront of New Deal reform because little room existed for privately run, modest, localistic, and model-type programs when federal intervention created public, large-scale, expensive, and national programs. Moreover, settlement house workers were amateurs in the days of the professional; they were involved in general catch-all activities when specialized agencies were increasingly assuming welfare responsibilities. Remember, too, that settlement house workers almost without exception supported the New Deal even if they did not provide leadership. And it is likely that the more active New York and Chicago workers were idiosyncratic, that their own prior political involvement and their location in cities where organized political action was far more prevalent (and not the absence of a community chest system) accounts for their response. Their settlements, after all, were no less dependent upon the good will of wealthy donors; the most Trolander can claim is that a decentralized funding system may have left them with a little more room for maneuver. Further, settlement houses in the 1930s were run by the second generation of workers, and in this case, as in so many others, the children of the founders seemed unable to match the energy and vision of the founders. Finally, the data marshaled to demonstrate the pressures that community chest leaders exerted on the settlement workers is skimpy, a handful of incidents and an occasional cautionary note by the workers themselves.

Trolander would have been far better advised to drop the simple formula of chest versus nonchest cities and explored in broader fashion the fate of this progressive institution throughout the depression. Instead, she is in the awkward position of trying to prop up a falling structure, until finally it collapses around her. Institutional histories must look outward, as well as inward. The fate of the settlement house reflects not only its own particular organization, but developments in the larger society. Had Trolander kept this more in mind,

her diligence in research might have enabled her to present a more balanced and ultimately a more persuasive book.

DAVID J. ROTHMAN  
Columbia University

IRVINE H. ANDERSON, JR. *The Standard-Vacuum Oil Company and United States East Asian Policy, 1933-1941*. Princeton: Princeton University Press. Pp. xii, 260. \$12.50.

Irvine H. Anderson, Jr. supplies another dimension to the study of the formulation of United States Far Eastern policy by focusing on the role of the Standard-Vacuum Oil Company prior to Pearl Harbor. Anderson shows how Stanvac, Royal Dutch-Shell, the State Department, and the British Foreign Office, though operating from different motives, developed a close working relationship in the 1930s as they tried to cope with the Canton Kerosene War, the Manchukuo Monopoly Law, the Japanese Petroleum Industry Law, and the Sino-Japanese War. Thus there was a firm foundation for cooperation when Stanvac shifted its attention after 1938 to the Netherlands East Indies and depended increasingly on the State Department to protect its stake in the islands. Realizing that an embargo would force Japan to seize the Netherlands East Indies, Stanvac and the State Department adopted a moderate form of oil diplomacy through the summer of 1941. The freeze of Japanese assets in July 1941 resulted in an unplanned *de facto* embargo only because an unguided bureaucracy took control of the process of implementation; continued Japanese aggression then prevented the policymakers from dismantling the embargo and thereby forced Tokyo to choose war. Recently Jonathan Utey has substantiated this point in greater detail and has singled out Dean Acheson as quarterback of the administrative run-around. In any event, Anderson demonstrates how Stanvac and Shell coordinated their exports from the Netherlands East Indies to make the embargo complete.

Using Michel Crozier's theory of bureaucratic power as an analytical tool, Anderson concludes that as Stanvac lost freedom to make administrative decisions, its role shifted from instigating diplomatic action by the State Department to implementing the policies of the Roosevelt administration. Anderson has drawn on extensive government and private manuscript records in the United States and England to document his thesis that petroleum diplomacy in East Asia involved a complex and dynamic bureaucratic relationship that defies simple generalizations.

Relying on studies by James Crowley and Akira Iriye, Anderson adds a few more pieces to the



puzzle of the causes of the Pacific war rather than providing a new conceptualization of the Japanese-American confrontation. While he holds the United States and Japan equally culpable for the war, some of his evidence underscores the cautious and defensive reaction of the United States to Japan's aggressive thrusts. Overall, Anderson has produced a thoughtful study that combines valuable information with a perceptive analysis of the relationship of the oil companies to the United States government.

NOEL H. PUGACH  
*University of New Mexico*

BENNETT H. WALL and GEORGE S. GIBB. *Teagle of Jersey Standard*. New Orleans: Tulane University. 1974. Pp. xxii, 386. \$12.00.

Here is an uncritical biography of Walter Clark Teagle (1878–1962), director of Standard Oil (New Jersey) from 1909 to 1937. Teagle also served Jersey, the oil industry, and the country in varied capacities: as manager of the Republic Oil Company; as president of Standard Oil of Ohio, the West India Oil Company, and Imperial Oil Company, Ltd. of Canada; as head of the Export Trade Department (Jersey); as adviser on the National Petroleum War Service Committee (1917–18), the Industry Advisory Board (1933), the National Mediation Board (1933), the Business Advisory Council of the NRA (1933–34), and the National Defense Mediation Board (1941). He also directed the Share-the-Work program under Herbert Hoover (1932) and administered the Teagle Foundation (1944–).

Having acquired the virtues of perseverance and hard work, Teagle fused a great capacity for detail with a prodigious memory. He mastered every phase of the oil business through experience as well as by reading oil trade journals. He worked with a deep sense of humility, conveyed a useful air of sincerity, remained accessible to all, and developed an ability to listen and heed opinions. He generated in Jersey employees a real feeling for the company. Even his hobbies provide evidence of thoroughness and enthusiasm.

His accomplishments were basic. He acquired oil producing properties and therefore changed both the structure and direction of Jersey and its affiliates. He revived Imperial in Canada; merged Jersey and Socony into Standard-Vacuum Oil in the Far East; marked profits in Sumatra, Peru, Colombia, Venezuela, and Mexico; and reorganized Jersey into a vertically integrated, professionally managed corporation. He pioneered in conservation, in employee-public-stockholder relations, and in scientific research. Though Jersey failed to show profits in Ecuador, Argentina, and

Bolivia, Teagle can be held responsible only for failure in Poland. He shouldered too great an administrative burden and delegated too little authority.

The book throughout contains exaggerations, allegations, unsubstantiated statements, and errors. Footnotes are lacking. (Citation of all interviews and specific documentation—especially archival sources—would have been helpful. Many questions are left unanswered, such as those concerning Jersey's "amateurish" public relations and Teagle's contradicting philosophies of efficiency in conservation and profits. It is surprising that a substantial part of the volume is devoted to Teagle's hunting and fishing interests. There is inadequate treatment of the 1930s, especially with reference to official duties at Jersey.

Yet there is much to commend this book. It is highly readable. Gems of detail sparkle throughout: for example, when Teagle sold Henry Ford his first contract for gasoline. There is good flow organization from business to private life. Teagle's relations with other oil titans—especially Henri Deterding and Heinrich Riedemann—are well presented. Finally, the authors convincingly demonstrate that no man in the history of Jersey Standard, other than John D. Rockefeller, had been in a position of comparable power, influence, and responsibility. Alas, the authors err in presenting Teagle as a man alone, omnipotent and omniscient.

FRANCIS J. MUNCH  
*American Institute of Petroleum Studies*

ROBERT SKLAR. *Movie-made America: A Cultural History of America Movies*. New York: Random House. 1975. Pp. vii, 340. \$12.95.

Movies were the most influential medium of culture in the United States during the first half of the twentieth century, but historians have largely ignored their enormous impact upon American society. The publication of Robert Sklar's *Movie-Made America: A Cultural History of American Movies* will make it difficult to continue this neglect.

Sklar's penetrating study begins with an analysis of the social status of the earliest American film makers and their audiences. Working-class immigrants dominated motion-picture production by the beginning of World War I, and urban workers and the poor became their patrons. Sklar explains that "movies were the first medium of entertainment and cultural information to be controlled by men who did not share the ethnic or religious backgrounds of the traditional cultural elites. . . . They rose to the surface of cultural consciousness from the bottom up."

This is one of the reasons, Sklar argues, that the

educators, clergymen, and middle-class reformers who served as caretakers of American culture were hostile to movies, and also why they constantly attempted to bring movies under their control by censorship. Sklar's suggestion that class considerations of social control through mass communications determined the nature of censorship campaigns is one of the most provocative theses of his book.

Sklar's discussion of the organization and business tactics of the movie trade makes fascinating reading, and his analysis of how both the screen and off-screen behavior of movie idols affected manners and morals is a significant contribution to our understanding of American society. The stars of Hollywood, he asserts, "were a vanguard of the increasingly larger role sexual openness has played in American public behavior during the past half-century."

Breaking with the conventional wisdom that television is to blame for Hollywood's post-Korean War financial crisis, Sklar persuasively argues that the industry's frightened response to the Hollywood witch hunts during the McCarthy era is the culprit. He suggests that the movie moguls "tried to avoid making movies that would offend any vocal minority. As a result they lost touch both with their own past styles and with the changes and movements in the dominant culture."

Sklar is at his best discussing the silent era, Disney and Capra, and the entrepreneurs who made Hollywood the world's dream factory. His treatment of post-World War II American cinema, however, is much less satisfactory. It is as if everything that happens in Hollywood after 1946—the industry's record-setting attendance year—was anticlimactic and unimportant.

In spite of this flaw, *Movie-Made America* is the best single-volume history of the role that movies have played in our society, and it is one of the most important contributions to twentieth-century American cultural history published in recent years. Sklar's excellent prose style and exceptional storytelling ability make this fine book an attractive supplemental reading for twentieth-century United States history courses.

MICHAEL R. GRECO  
University of Houston,  
Clear Lake City

JAMES J. FLINK. *The Car Culture*. Cambridge: MIT Press. 1975. Pp. x, 260. \$14.95.

Surveying the impact of the automobile, or what he prefers to call "automobility," upon American life, James J. Flink views with disfavor the effect on the country's culture of the universal adoption of

the automobile. Those who are familiar with the writings of such critics as Emma Rothschild, John Kenneth Galbraith, and Robert and Helen Lynd will find little here of a new or controversial character. Flink's synthesis of the case that these and other critics have made against the "car culture," however, especially for the period since World War II, provides a useful counterbalance to the scholarly defense of this culture by John B. Rae, another automobile historian.

A basic feature of Flink's interpretation is his assertion that Americans enthusiastically embraced the automobile from the beginning and that it is only "a popular myth" that the early auto manufacturers had to fight a strong tide of criticism and opposition to sell their product. It was only later, in the twenties, that some voices began to be raised opposing the direction the automobile was taking the country, but by then the average American was so obsessed with the desire to own a car that he was an easy mark for the sophisticated tactics manufacturers used to make the car ever more essential to daily life. But an examination of the materials on the early history of the automobile, including manuscript and newspaper records, as well as the periodical sources which Flink used, reveals that the case against the automobile was made on numerous occasions in the 1890s and early 1900s. Americans adopted the automobile not because they were unaware, for example, of the noisy, air-polluting character of the gasoline engine, but because the alternative seemed then, as it would to most of us today, less desirable.

The problems that grew out of the decision made by Americans at the beginning of this century are effectively summarized by Flink, but like most of the critics of the car culture he presents no real alternative until his next to last sentence when he maintains that a "future characterized by true community and expanded democracy, free from the privatism, materialism, escapism, and exploitation that the automobile culture encouraged, is . . . within our grasp." Perhaps the details of how that golden age may be attained will be forthcoming in a later book.

GEORGE S. MAY  
Eastern Michigan University

MARSHALL SMELSER. *The Life that Ruth Built: A Biography*. New York: Quadrangle. 1975. Pp. xiv, 592. \$12.50.

There has been, and still is, a reluctance within the profession to admit that such "frivolous" matters as baseball merit scholarly consideration. Historians who undertake serious studies of sports or sports figures are invariably dismissed as betrayers

of the faith and are banished from the guild. Usually, however, when new routes to historical understanding are being charted, the initial doubts give way slowly to acceptance and then approval, until ultimately the pioneers are acclaimed for their feats. Sports history is moving boldly ahead and will in time establish itself as a significant subdiscipline.

We have new support for this view in the first baseball biography by an academic historian. Marshall Smelser of Notre Dame comes to his task with impeccable credentials. A specialist in the early national period, he has written half a dozen volumes, including *The Winning of Independence* and *The Democratic Republic*. In his latest work he demonstrates that a good historian can write good sports history and that good sports history makes good history. Of course Smelser had an ideal subject: Babe Ruth, the Sultan of Swat, the Colossus of Clout, the Bambino. But the author made the most of his opportunity.

The Babe has enjoyed a rebirth of glory in recent times, and a number of works on him—most of them by sportswriters—have flowed from the press. They are fairly good books, but they suffer from a journalistic glibness and a flippant style.

Smelser writes, as he says, a "baseball book for people," not a "book for baseball people," and it stands above all others in the field. The research is extensive. The author has read widely in the newspaper press—the only real archival material!—interviewed over sixty former teammates of Ruth, and consulted hundreds of baseball experts. He has organized his subject matter chronologically, interspersing the narrative with timely scientific analyses of diverse aspects of the game. Of course, in the center is the Babe, potbelly and all: his strengths, weaknesses, sources of popularity, illnesses, achievements, unhappy first marriage, happy second marriage, and sadness after retiring.

One particular strength of the book is its balance and moderation. Smelser says in the preface that "I don't deny Ruth's gluttony, but I tried to get it straight and explain it." He succeeds admirably. In an age when many journalists and historians appear to delight in debunking celebrated figures for no useful purposes, Smelser maintains a commendable sense of propriety without compromising his honesty. Of course, Ruth was a glutton, but he was also a human being who had an unfortunate childhood. The historian's task is to understand sympathetically, not judge heartlessly. Smelser is in the best tradition.

The periodic boldface quotations may be disconcerting, and the absence of footnotes and a detailed bibliography—for reasons of economy—

might be disappointing, but still this is the best Ruth book to date. A scholarly sport biography? Hurray!

EUGENE C. MURDOCK  
*Marietta College*

SEYMOUR MARTIN LIPSET and DAVID RIESMAN, *Education and Politics at Harvard*. (Two essays prepared for the Carnegie Commission on Higher Education.) New York: McGraw-Hill Book Company. 1975. Pp. xi. 440. \$15.95.

The relationship between this book and historical scholarship is indirect and ambiguous. The second section, a long essay by David Riesman comparing Harvard College in the late 1920s, when he was a student there, to Harvard College since 1958, when he joined its faculty, is written in the style that we have come to expect from him—that of an insider, privy to information and insights from which others are excluded, yet not so much an insider as to lose all sense of objectivity in the delight of sheer belonging. Riesman would like to combine the cool, quizzical detachment of George Santayana with the passion and analytic power of William James. He does not succeed; no one could, but it is a tribute to him that he puts us in mind of such giants, and historians of Harvard University in the future will be grateful to him for having presented them with this set of intelligent anthropological field notes on the degree to which the culture of the natives of Harvard Yard has become "meritocratized" since the 1920s.

The first and longer essay by Seymour Martin Lipset is an attempt at writing a history of political controversy at Harvard—controversy among the constituent elements of the university and between the university and the larger community—but it is an attempt that violates almost every canon of historical method and many of general scientific inquiry as well. Limitations of space prevent the listing of all the errors, even of all the categories of error. Suffice it to say they run the gamut from sheer factual inaccuracy—Henry A. Wallace was not secretary of commerce in 1935—to inadequate use of available sources, to misinterpretation of documents, to *suggestio falsi* and *suppressio veri*.

Roughly the first two hundred pages, dealing with the history of Harvard until the 1960s, seem not so much written by Lipset as lifted from the pages of monographs written by others. No manuscript sources have been used—not even those in the Harvard archives—and original sources even when they exist in published form, appear only when they have first been used as quotations in secondary accounts written by others. A statement that the "key year" for revolutionary politics at

Harvard "seems to be 1768 when the records tell us of the 'liberty tree' or 'rebellion elm' in Harvard Yard" rests on a two-sentence statement from the sister of Abigail Adams, quoted not from the source but from S. S. Cohen, "Student Unrest in the Pre-Revolutionary Decade, 1765-1775" (unpublished paper, Department of History, Loyola University, Chicago [1971]). Cohen's teachers, I am sure, required more careful use of documents. The records of the Harvard Corporation are available; why then, does Lipset quote them from E. J. Kahn, Jr., *Harvard through Change and through Storm* (New York [1969])? Lipset's method precludes the possibility of doing more than provide a terminology to apply to the events in his chronicle. Unless others have dug up the quotations, he cannot use historical knowledge to check the validity of his own hypotheses or to suggest new problems for investigators.

But even if he had gone to the sources himself and done his own historical reconstruction, it is doubtful, at least on the showing in this book, that the situation would have been much improved. The plain fact is that Lipset simply does not read documents correctly. A statement by Charles Francis Adams that though Harvard in the 1850s had "a set of rather eminent scholars . . . it never entered into the professorial minds" to give direction to their students, is treated by Lipset as if it were a complaint by Adams that Harvard did not have "important scholars." Lipset seems to rest his statement that "Harvard, which had a few Jewish senior professors in the early thirties, may have been far from the most discriminatory in this respect as well" on the quoted conclusion of a report by the American Jewish Committee that says nothing whatever about Harvard. On the basis of that evidence, we are entitled to draw no conclusions whatever concerning how discriminatory Harvard was.

More disquieting is the way in which Lipset builds his case, not on logical inferences from documents, but by suggesting through juxtaposition and by explaining away facts that are not easily accommodated by his thesis. Students are Lipset's particular *bête noire*, and his delight in pointing out their errors sometimes leads one to wonder whether he can hear the difference between his own heartbeat and the crack of doom; writing of the decline of isolationism at Harvard, he says that "before December when the Japanese bombed Pearl Harbor, the students of Harvard, by an overwhelming majority, had shown they had learned that their President [Conant, not Roosevelt] and professors had been right." What appears in the evidence to sustain the charge that after World War II Harvard students "revealed themselves as believers in cooperation with Com-

munists"? The Liberal Union set up a booth to protest the end of price control and boycotted Harvard Square merchants who raised prices. When students are politically to the right of the faculty, as they were at Harvard during the New Deal, they are twitted for their conservatism. When they are to the left of the faculty, as they were in 1949 when polls showed that while the faculty voted two to one against allowing Communists to teach, students favored the proposal in the same ratio, Lipset minimizes the embarrassing fact; the students were not "active" in the fight for civil liberties of Communists and besides, Norman Thomas was against the idea.

Justice Holmes once said that the point of deciding a case was to allow us to get on with the business of living. For Lipset, the writing of history is not so much to create a fellowship of understanding as to perpetuate the conflicts that should be understood. There is an entry in the index that may explain this quality of the essay: "Narcissism, interfering with curiosity."

SIGMUND DIAMOND  
Columbia University

PAUL MERKLEY. *Reinhold Niebuhr: A Political Account*. Montreal: McGill-Queen's University Press. 1975. Pp. xii, 289. \$13.50.

The Reinhold Niebuhr literature is extensive. During his lifespan (1892-1971) Niebuhr wrote twenty books and over nine hundred occasional pieces. Students of his career have been equally prolific: for example, June Bingham has examined his life, Hans Hoffman his theology, Gordon Harland his mature thought, and Ronald Stone his works on foreign policy, etc. In addition, Niebuhr is probably the subject of more doctoral dissertations than any other twentieth-century religious figure. Given this abundance, one welcomes the terse and witty book under review. It is the most satisfactory study of Niebuhr now available.

In the preface Merkley asks that the book be read "as a contribution to political history." This is a rather peculiar request, for he treats religious issues as much as political ones. In fact, Merkley's major point is that the religious and political positions of Reinhold Niebuhr are inseparable. In the early 1930s, with the discovery of both Marxism and European "crisis theology," Niebuhr moved simultaneously to the political left and the theological right. Thus, those whom Morton White once termed "atheists for Niebuhr" simply did not understand their master.

This volume stresses the years 1920-50, but the summary chapters are especially well drawn. Merkley suggests that during Niebuhr's lifetime, he "did much good and a certain amount of mis-

chief." The mischief came primarily from his critique of Russia's "hard" utopian heresy, for by 1949 Niebuhr had become almost a cold-war ideologue. Yet Merkley finds that the good far outweighs the mischief: Niebuhr interpreted and modified European theology for two generations of American ministers. He reminded policy makers, from George Kennan to Hubert Humphrey, that nations are seldom as moral as they profess to be. He revived the Biblical ideas of original sin, paradox, and mystery. Moreover, from his preaching years in Detroit to the publication of his complex *Nature and Destiny of Man* (1943), he insisted that theological ideas be applied. For five decades Niebuhr reminded the nation that political discussion depends upon ethics, and ethics depends ultimately upon theology. This message has not lost its relevance.

FERENC M. SZASZ  
*University of New Mexico*

PATRICK ABBAZIA. *Mr. Roosevelt's Navy: The Private War of the U.S. Atlantic Fleet, 1939-1942*. Annapolis: Naval Institute Press. 1975. Pp. xii, 520. \$18.50.

As a narrative of operations in the Atlantic to mid-1942 by units successively known as the Training Detachment, Atlantic Squadron, Patrol Force, and Atlantic Fleet, this revised 1972 Columbia dissertation is the most comprehensive in print. It rests on a wider array of sources than was available when Samuel E. Morison published *The Battle of the Atlantic, 1939-1943* in 1947, and with a text one-fourth longer than the relevant pages in Morison, it more fully covers developments before the outbreak of the European war. Very useful, for example, is the analysis of Fleet Problem XX in February 1939. Patrick Abbazia's main concern is to convey a feel for the problems and frustrations of antisubmarine and escort actions, which he does expertly. He also offers authoritative accounts of events in 1941 left unclear by earlier writers—the planning of landings in Martinique and the Azores, the secret transfer of ships from the Pacific Fleet, the on-again off-again institution of escort of convoys, and the controversial *Greer* incident. The bibliography reveals an unprecedented use of the Classified Operational Archives of the Naval History Center, of naval records in the National Archives, of unpublished administrative histories held by the Navy Department Library, and of interviews with and letters from men who were junior officers in the Atlantic in 1939. The book contains twenty well-chosen illustrations for which, however, there is no listing in the front matter. The one general map and the end papers tracing the route of two convoys are insufficient for even knowledgeable readers.

As a contribution to understanding Roosevelt's tortuous foreign policy and military strategy, the volume is less successful. For events in Washington the author relies on the printed record, which he has not completely mastered, and leaves untouched the riches at the Roosevelt Library in Hyde Park and at other regional repositories. At the Library of Congress he limited his investigation to the papers of Secretary of the Navy Frank Knox and of Admiral William D. Leahy. He did not examine the State Department archives for papers omitted from the *Foreign Relations* volumes, which for those years did not always include documents dealing with interdepartmental discussions or security issues. Abbazia never comes to grips with the question of whether Roosevelt decided in September or October 1941, as some authors contend, that to effect the defeat of Germany he must go beyond the policy of all aid to England short of war and bring about the total belligerency of the United States.

RICHARD W. LEOPOLD  
*Northwestern University*

J. A. STOCKFISCH. *Plowshares into Swords: Managing the American Defense Establishment*. New York: Mason and Lipscomb. 1973. Pp. xi, 328. \$12.50.

This book critically examines the weapon selection and procurement process in the United States, indicates flaws in the present system, and prescribes certain changes. The shortcomings of the current weapon procurement system, as well as broader issues of military management, are considered within the framework of a theory of political-bureaucratic behavior. The historical evolution of the weapon development process in the United States is discussed with considerable attention to European experiences and precedents.

This is not an overview of the American defense establishment, or a comprehensive survey of defense decision making. It is an enlightening analysis of some problems that arise in the management of a large, costly military establishment in an era of rapid technological change. Although understanding of this complex topic should be enhanced for all who read this book, those who are familiar with post-World War II defense policy will profit most from the author's many useful and provocative insights.

The author, a staff member of the RAND Corporation and a former research associate at the Institute of Defense Analysis, is primarily concerned with the need to increase the efficiency with which resources are allocated and utilized for weapons procurement in the United States. A major problem in this regard, he insists, is the "technological syndrome" that afflicts the procurement



process—the tendency to equate improved technical performance with enhanced combat effectiveness. Thus, expensive weapon systems, which may be of limited utility in combat, are developed and produced. J. A. Stockfisch devotes considerable attention to the testing process, which, he contends, relies far too heavily on computer simulations with their structural and operational-data deficiencies. He advocates more and better quality field testing and operational evaluation, that is, more of the World War II type of operational research, and he offers detailed suggestions for organizational and procedural changes to achieve that objective. More fundamentally, he proposes that the military services be given more aggregative budgetary freedom (within the confines of a smaller total budget) as a means of increasing incentives to improve efficiency in the development and use of weapons, as well as the management of defense more generally. Arguing that the present system of Congressional control over detailed line-item military budgets creates and sustains “budget maximizing incentives” for the military services, Stockfisch suggests that Congress confine itself to dealing with major force-structure categories and issues with high foreign-policy content, thus providing the services with the opportunity and incentives to allocate resources in an efficient way.

The author’s proposal is persuasively argued and deserves serious consideration. The reader may be less than sanguine, however, about the potential of such a shift in budgetary power to increase the efficiency of defense management and reduce defense expenditures. Such large scale institutional changes are not easily made, as the author readily admits. Even if changes could be made along the lines suggested, there is little evidence available to insure that the desired result would be achieved. Perhaps what the proposal needs is the kind of operational testing that the author prescribes for weapon system models.

This book is a valuable contribution to the literature on defense management and an excellent demonstration of the role of bureaucratic influences on defense expenditures. As such, it must be considered with other works that perceive the source of high levels of defense spending elsewhere.

ELIZABETH CRUMP HANSON  
*Yale University*

E. RAYMOND WILSON. *Uphill for Peace: Quaker Impact on Congress*. Richmond, Ind.: Friends United Press. 1975. Pp. xx, 432. \$7.95.

“It is incumbent upon the individual not only to try to be good, but also, individually and in concert with others to try to be effective.” Such has

been the rationale from its inception of the Friends Committee on National Legislation, of which the author was the initial executive secretary. At all levels of the legislative process, from committee hearings and bill-drafting to presidential interviews, these Quaker lobbyists have made their presence felt, eloquently and persistently, for the past thirty-three years.

The range of activities, in state legislatures as well as upon Capitol Hill, that the FCNL has influenced is impressive: efforts to feed the hungry, ranging from war relief in the early forties to the War Against Hunger proposals of the mid-1960s; proposals to admit refugees into the United States more generously than in the recent past; the defense of civil liberties and the rights of minorities; the commitment of America to international cooperation; opposition to militarism and preparation for disarmament; the lessening of the potentially or actively violent overseas commitments the United States has undertaken in the past three decades. These are matters upon which Americans were (and are) passionately divided. To lobby on one side of any such issue is to raise serious First Amendment questions, as the author is well aware. Whether a tax-exempt religious organization, constitutionally speaking, can lobby for its beliefs under the “free exercise” clause without violating the “establishment” clause is a deeply troublesome dilemma, and to resolve it the contributors to the FCNL have had to forego tax exemption.

In the view of these Quaker activists, witnessing to one’s convictions, whether noisily or quietly, is not enough; one must seek to make things happen. The Quakers also, however, reject the politics of confrontation of the previous decade. Some who have struggled through those confrontations may find the record of all this buttonholing and letter-writing and petitioning a bit old-fashioned. The institutional mind-set of this book is expressed in the author’s description of the May days of 1970 that followed the Cambodian invasion—marches, classroom strikes, and deaths—as “the busiest telephone week in FCNL history” (p. 310). On the other hand it would be a sanguine radical indeed who would now maintain that the marchers and confronters accomplished more.

The author has drawn upon the FCNL’s archives, housed in the Swarthmore College Peace Collection. This is the testimony of an organization man, and one should not fault it for not being a scholar’s monograph. It will be of great utility for those who do write such monographs, once the nostalgia for the years when people shouted “Right on!” (as an updated synonym for “Amen, brother!”) has had a little more time to fade.

PAUL A. CARTER  
*University of Arizona*

DAVID M. OSHINSKY. *Senator Joseph McCarthy and the American Labor Movement*. Columbia: University of Missouri Press. 1976. Pp. 206. \$10.50.

The title aptly describes this monograph's restrictive focus with one exception: this is principally a study of the labor movement's leadership. David M. Oshinsky first considers labor's role in LaFollette's 1946 primary defeat and in McCarthy's final senatorial victory, reviews the Wisconsin senator's record on issues of concern to labor during his pre-Wheeling days and the responses of the CIO and the AFL to postwar issues and to McCarthyism, and then assesses the origins of McCarthyism and its impact on the American labor movement. The result is a solid, well-written, narrative history that refutes traditional interpretations of McCarthy's 1946 Senate victory and provides important insights into the labor movement's changing politics and priorities during the early cold-war years.

It is, however, a somewhat disappointing study. Because Oshinsky focuses so narrowly on McCarthy and the labor leadership, he fails to explore rank-and-file attitudes toward the senator and toward McCarthyism. His documentation consists principally of labor publications, the proceedings of the major unions' national conventions, and electoral statistics from Wisconsin labor wards (but exclusively percentages of total votes, as he does not meaningfully analyze these votes by relying on more sophisticated statistical techniques such as factor analysis and correlation coefficients). And, by focusing on McCarthy instead of McCarthyism, Oshinsky only tangentially addresses the way in which labor and national politics were transformed during the cold-war years. Our understanding of McCarthyism is not significantly extended; and we learn little about the politics of the Taft-Hartley and the Communist Control Acts or about labor's changing positions toward foreign and internal security policy issues. These are far more substantive questions than learning how the labor leadership reacted to the Wisconsin Senator's specific charges.

Nonetheless, this is a valuable study, one that students of cold-war politics can profitably consult to understand better the contentious anti-Communist politics of the postwar years.

ATHAN THEOHARIS  
Marquette University

EDWARD L. SCHAPSMEIER and FREDERICK H. SCHAPSMEIER. *Ezra Taft Benson and the Politics of Agriculture: The Eisenhower Years, 1953-1961*. Danville, Ill.: Interstate Printers and Publishers. 1975. Pp. xviii. 374. \$6.50.

Did Eisenhower's Secretary of Agriculture Ezra Taft Benson change the direction of agriculture at

important moments in American agricultural history? In contrast to some negative pre-Watergate analyses, Edward L. and Frederick H. Schapsmeier answer yes with an interesting, challenging monograph based upon the Benson papers and other major manuscript collections.

The Schapsmeiers portray Benson as a dedicated, refreshingly honest individual who manifested his religious faith and talents in the Department of Agriculture. A reformer encountering vitriolic criticism, Benson attempted to reverse previous agricultural policies, increase open-production and free-market policies, and reduce price-support levels. As secretary, he adhered to principles and resisted short-range political expediencies. The authors reveal Benson's influence and those of his associates in the major agricultural events, including the Agricultural Act of 1954, efforts to distribute farm surpluses by using market-oriented research, the Agricultural Trade Development and Assistance Act, and the Soil Bank and the Agricultural Act of 1958.

But like the secretary himself, the book suffers when it treats with Congress. To accept the view that Congress prevented needed long-range agricultural reform, one must understand that Benson's personality limited his effective practice of the art of compromise and his understanding of the ways and means of Congressional and agrarian politics. "Great" secretaries of agriculture must manifest political astuteness, and the book could have been improved with a more sophisticated portrait of congressional agricultural action.

*Ezra Taft Benson and the Politics of Agriculture* presents other interesting questions for scholars to ponder. Was Eisenhower an "artist in iron" for moderate agricultural policies? To the authors' credit, they wrote a scholarly book about Benson who is still alive; yet did this challenge restrain their criticism when they delicately wrote of the secretary's conservative political associations? Will the actions of future secretaries of agriculture fully substantiate the thesis that Benson broke "through the inertia of established tradition and entrenched attitudes to show the way toward agricultural reform" (p. 274)? Was Benson a "great" secretary of agriculture as the book advocates?

The authors have heaped up a bountiful plate of food for thought. Their book will provide sustenance for future studies of agricultural politics during the Eisenhower administration.

IRVIN MAY, JR.  
Texas Agricultural  
Experimental Station

FRANK J. SORAU. *The Wall of Separation: The Constitutional Politics of Church and State*. Princeton: Princeton University Press. 1976. Pp. xiii, 394. \$15.00.

While this book is not history in any usual sense, historians and political scientists can read it with profit because it covers ground usually left unexplored by legal scholars. Its purpose is to examine the individuals and groups involved in cases concerning the separation of church and state between 1951 and 1971. It thus includes analysis of those who bring such cases to court, of those who intervene, of the lawyers, defendants, and judges who participate. There is extensive and knowledgeable discussion of legal strategies developed by the major separationist groups, such as the American Civil Liberties Union. Frank J. Sorauf does not attempt to deal with the policies emerging from the decisions or, except briefly, with their impacts.

The value of the book is thus judged by whether it enhances our knowledge of how policy emerges from the legal process in the course of constitutional litigation in the church-state field and, by implication at least, in constitutional law generally. Many of its conclusions are readily transferable, especially to other issues in civil rights and civil liberties.

The book is well written and thoroughly researched. It displays sound scholarship and mature judgment and will be a major volume in the developing field that the author calls "constitutional politics"—the area in which courts, by ruling on constitutional issues, inevitably make public policy. Although at some points the author refers to the courts as "passive," he demonstrates that the policy which emerges from courts is likely to be the policy favored by the judges involved, almost regardless of the constitutional language they are construing. Thus they are anything but passive.

The book is exemplary in most respects and truly enlightening in some, such as the data indicating that avoidance of trial or of a process that really brings out the facts sometimes has important effects on the results. There are, however, some fairly important caveats. Sorauf often concentrates on "the actors" so narrowly that one almost feels that he does not realize that both plaintiff and defendant usually represent large and significant social groups and forces; actors tend to appear much more lonely and idiosyncratic than they actually are. He is unusually diffident in his attempts to figure out why judges have particular policy preferences, sticking primarily to a rather cursory analysis of external influences and ignoring personal value preferences even when, as with Hugo Black or William O. Douglas, they are quite obvious. He tends too easily to assume that "the majority" opposes separationist policy, and he regards the hostile acts of crackpots as representative of majority views. He apparently thinks that *Engel v. Vitale* outlawed school prayers, so that

states which kept them after that decision were "noncompliant." And he fails to acknowledge that a state legislature may react to a separationist decision by doing nothing at all—which is how most states actually have reacted.

Despite such lapses, this is an excellent book, which deserves a place next to Wood's treatment of child labor or Vose's of the race cases.

LOREN P. BETH

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## CANADA

FRANK H. EPP. *Mennonites in Canada, 1786-1920: The History of a Separate People*. Toronto: Macmillan. 1974. Pp. xii, 480. \$9.95.

JAMES C. JUHNKE. *A People of Two Kingdoms: The Political Acculturation of the Kansas Mennonites*. (Mennonite Historical Series.) Newton, Kans.: Faith and Life Press. 1975. Pp. xii, 215. \$7.95.

These two very different books on the North American experience of the Mennonites converge at only a few points. Both focus on Mennonite acculturation. Both climax with the immense difficulties Mennonites faced in Canada and the United States during World War I. Finally, both authors are "progressive" Mennonites, with great sympathy for the Anabaptist tradition, yet without any evident concern for the vital and often subtle theological beliefs that alone have given sense and meaning to distinctive Mennonite institutions.

James C. Juhnke's *People of Two Kingdoms* is a very narrow, restricted, largely political study of Russian Mennonites in Kansas. He begins with some surprisingly inconclusive data on probable Mennonite voting patterns in ten Kansas townships. Since the Anabaptist doctrine of a pure and holy church precludes either voting or office holding by Christians, it is not surprising that these heavily Mennonite towns revealed a lower level of political participation than did non-Mennonite areas of Kansas. What is surprising is that, as Juhnke interprets his data, the extent of Mennonite voting and office holding seems to have shifted very little from the first decade (1880s) after immigration to the present, and only in recent decades have Mennonites revealed any hint of bloc voting. Unfortunately, his raw voting data do not support any firm judgments at all. Three different census returns reveal that from twenty-eight to fifty percent of the residents of his selected towns had non-Mennonite surnames. This at least blurs any voting patterns among Mennonites. Even surnames reveal little, since Juhnke never tries to correlate them with church membership lists or other, more subtle tests of orthodoxy.

It is fortunate that Juhnke quickly turns from

voting returns to other indicators of cultural change, change that he inconsistently applauds when it meant greater Mennonite political involvement, and deplores when it entailed any compromise of the related principle of nonresistance. Consistent with these judgments, his brief and shallow survey of Mennonite history offers much more of a contextual than a doctrinal explanation of Mennonite separatism. In the changed American context, he discovers varied evidence of Mennonite assimilation of typical American values. His primary focus in several brief chapters is on the Mennonite political response—their acceptance of American citizenship, their continuous dialogue about politics, their participation in local government, their response to public schools, their susceptibility to political demagogues, and, above all, their extended efforts to gain government recognition for their extreme form of pacifism. He attends only briefly to what may be the most erosive cultural challenge faced by Mennonites in America—the beguiling beliefs and practices of evangelical Protestants. Religious acculturation contributed to what Juhnke deplores: extreme fragmentation and thus a complex, schismatic, institutional history that he briefly characterizes, but rarely clarifies.

Frank H. Epp's book, the first of a projected two-volume history of Mennonites in Canada, is as broad as Juhnke's book is narrow. The early chapters on Mennonite history and early settlement in Canada are the weakest of all. They are simplistic in both conceptualization and explanation, theologically barren, episodic and anecdotal in detail, and confusing in chronology. But once Epp gets the Mennonites established in Canada, he offers a detailed, at places almost antiquarian, chronicle, with scarcely a significant leader or a single congregation overlooked. Detailed maps and tables locate and classify almost all Mennonite (or Amish) congregations. About half the book is very tedious, institutional history. Epp identifies the origins, in Russia or the United States, of all the affinity groups or emerging sects among Mennonites at the time of migration to Canada and then follows up each new schism and each new effort at unification. The complex story involves four or five major Mennonite denominations or groupings and about a dozen other small sects, some involving only two or three congregations. The details are confusing, but Epp is attendant to all the divisions (he deplores them), and he tries to be fair to the whole spectrum of Mennonite opinion and practice, from the rigidly orthodox and separatist Old Colony Mennonites to the larger, more assimilationist General Conference Mennonites or the Mennonite Brethren.

Epp, almost as much as Juhnke, also emphasizes

cultural conflict. He tells this story with obvious affection for the Mennonites, with a sympathetic understanding of the pressures for Canadian nationalism but without any extensive analysis or any attempt at careful explanation. Because Canada offered Mennonites fuller assurances of military exemption than the United States, it attracted the most rigidly orthodox of the Russian Mennonites. But Canadian institutions quickly challenged the relatively autonomous way of life the Mennonites had known in Russia. Except for the Old Colony, Canadian Mennonites were unable to maintain block settlements and village customs. They lost members to other churches and vainly fought against political participation by church members. Even their military exemption provoked intense hostility during World War I and led in its aftermath to a law excluding further Mennonite immigration. It is at this point of crisis and conflict that Epp closes volume 1, a volume that reveals mastery and compelling drama only in its closing chapters.

PAUL K. CONKIN  
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Madison*

RICHARD VEATCH. *Canada and the League of Nations*. Buffalo, N.Y.: University of Toronto Press. 1975. Pp. xi, 224. \$15.00.

At the end of the First World War, the Canadian prime minister, Sir Robert Borden, successfully insisted upon Canada's being accorded the right to sign the Treaty of Versailles and to become a member of the newly formed League of Nations. His motive was to secure recognition for his country as an emerging nation.

In this historical study of Canada's role as a member of the League, the author, a professor in the political science department at the University of Winnipeg, develops the theme that Canada's principal objectives at Geneva were to consolidate her "fully independent status . . . at the international level," to minimize "as much as possible Canada's commitments under the collective system," and to insist that the League should not become involved in questions, such as tariff and immigration policy, that Canada regarded "as purely domestic matters." These being Canadian objectives, it may be said that Canada achieved some success in the League, even to the extent of successfully resisting the effort of the Hereditary Chiefs of the Six Nations to use the League as an instrument of intrusion into Canada's internal affairs.

The author then goes on to criticize Canada for following British policy instead of influencing it and for failing to adopt a more positive role by demanding effective economic sanctions against



Japan in 1931 and against Italy in 1935. In other words, he follows a line, almost traditional in Canada, of assuming that the newly emerged nation carried greater weight in world affairs than it really possessed. In his view, Mackenzie King and Oscar Skelton were two assassins of the League. From a more realistic standpoint, it is difficult to imagine that Britain, France, and Russia could take their cue from a country that only recently became a nation, particularly one with limited military resources.

Richard Veatch's study is based largely on archival sources in Ottawa and Geneva and on the personal papers of those who played a prominent part, such as King, Bennett, Lapointe, Riddell, and Skelton. His book is packed with information presented in a clear, well-organized manner. It should serve as the standard work in this field for some years.

GEORGE F. G. STANLEY  
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#### LATIN AMERICA

JAMES LOCKHART and ENRIQUE OTTE, translated and edited by. *Letters and People of the Spanish Indies: Sixteenth Century*. (Cambridge Latin American Studies, number 22.) New York: Cambridge University Press. 1976. Pp. xiii, 267. Cloth \$14.95, paper \$4.95.

The conquest and settlement of Spanish America in the sixteenth century has usually been written under the spell of the heroic and romantic image of the early chronicles, the official correspondence of bureaucrats, or the fiery denunciations of Las Casas' image of rapacious and blood-thirsty conquistadores. Skillfully using notarial records, James Lockhart, in his *Spanish Peru, 1532-1560* and *The Men of Cajamarca*, drastically revised that assessment. The conquistadores were not professional soldiers; among their ranks were representatives of all classes and most professions from noblemen to professionals, merchants, artisans, foreigners, and even a few blacks.

This book under review is a very successful attempt to broaden and deepen this new interpretation. The authors-editors have selected a sample of the private (as opposed to the official) correspondence of conquistadores, settlers and officials, and clerics: "Through their own communications they emphasize the extent and earliness of development, the endless variety of occupations and types. And they express even more directly the humanity, universality and ready intelligibility of themselves and of the whole process. There is hardly a word that would not fit equally well in the mouths of immigrants—and non-immigrants—much nearer to our time and place."

The most revealing section of the book concerns the variety of life in the Indies. Letters from an encomendero, a miner, a merchant, a professor of theology, a new arrival, a tanner, a troubadour, a woman, a farmer, a tradesman, a nobleman, and a Hispanized Indian illustrate in a human and poignant way the complex process of how in a few generations the whole fabric of Spanish society was transplanted to the New World.

What Lockhart and Enrique Otte apparently seek to do is demythologize this period. That their scholarship has enormously enriched our understanding is indisputable. The conquerors and settlers in their private correspondence emerge as ordinary men and women of flesh and blood. There is much immediacy, reality, sobriety, and simple informativeness in these letters. The heroic, romantic, and mythical dimension, so graphically expressed in the chronicles of this period, is virtually absent from this selection of correspondence.

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MARIO GÓNGORA. *Studies in the Colonial History of Spanish America*. Translated by RICHARD SOUTHERN (Cambridge Latin America Studies, number 20.) New York: Cambridge University Press. 1975. Pp. xi, 293. \$17.95.

COLIN M. MACLACHLAN. *Criminal Justice in Eighteenth Century Mexico: A Study of the Tribunal of the Acordada*. Berkeley and Los Angeles: University of California Press. 1974. Pp. viii, 141. \$9.00.

JAN BAZANT. *Cinco haciendas mexicanas: Tres siglos de vida rural en San Luis Potosí (1600-1910)*. (Centro de Estudios Históricos, Nueva Serie, number 20.) México, D.F.: El Colegio de México. 1975. Pp. x, 226.

The publication of a general overview by Mario Góngora must be regarded as an important event for students of Colonial Spanish America. The author is well known for his *Estado en el derecho indiano* (1951) and his *Encomenderos y estancieros* (1970), as well as for a number of articles in Chilean journals. Given the quality of his scholarship, he is less well known than he should be in the United States. The present book, an original publication in English, should do much to secure him a proper reputation here. Góngora's place in the literature is that of a historian of law and institutions who has sought to add new social and intellectual dimensions to standard topics. Thus he appears as a kind of Chilean counterpart to Silvio Zavala, and one can readily detect in his work the influence of the founders of our modern approach to colonial history: Marcel Bataillon,



François Chevalier, Juan Friede, Richard Knetzke, Guillermo Lohmann Villena, and, among Americans, Lewis Hanke and Woodrow Borah. Góngora's special procedure is to focus on a supposedly well-known subject, to describe and develop it in a low-key way, gradually to suggest new perspectives, and finally to emerge with an interpretation that casts the subject in a new light. It is an impressive and persuasive technique.

The American conquests, which have commonly been understood as derivatives of the state controlled aspects of *reconquista*, are here related to less-familiar features of Spain's pre-1492 expansion: the *cabalgadas*, or forays for quick booty, and the extrapeninsular activity in the Atlantic and in Africa. Distribution of booty takes on new and revealing implications when we realize how clearly it demonstrates the Spanish soldiers' assumptions concerning the rewards of conquest. Contrary to the familiar and popular notion of conquest as derring-do, there emerges from these essays a pattern of conquest related to the institutions of settlement. Economic development in Hispaniola and Cuba provided preconditions for the capital endeavors that the mainland conquests undoubtedly were. As services to the crown the conquests evoked expectations of royal reward in a derivatively feudal *quid pro quo*, and encomienda itself may be understood as a kind of booty or royal reward for conquest. Ideas such as these, developed, interrelated, and analyzed anew, comprise the substance of what Góngora has to say about conquest. Other topics, similarly treated, are the missionary empire, the state, native labor, the Enlightenment, utopian writing, and the periodization of Spanish colonial history. Obviously, it is an incomplete list if one's purpose is to survey the entire colonial period. But each item subsumes others, and a proliferating structure eventually embraces much more than the bare list of topics would suggest. Still we miss those subjects that are never considered, and I would mention especially, because they would lend themselves well to the Góngora technique, the Columbian discovery and the economy of the seventeenth century.

The tribunal of the Acordada, an eighteenth-century Mexican judicial court, is not mentioned by Góngora and has received very little attention from other historians. Colin M. MacLachlan's monograph traces Acordada history from its establishment about 1720 to its demise in 1812. The study is objective, scholarly, and factual. Nearly a third of it deals with the pre-Acordada context of law and criminality, in comparison with which the Acordada appears as a liberating institution, itself free (or relatively free) from political connections. Like the conquests (but with quite a different meaning) the Acordada was managed by ama-

teurs, and in the atmosphere of the late eighteenth century it may be understood as a law-and-order institution conducted by and for the interested parties.

Jan Bazant's study of five haciendas in San Luis Potosi adds one more item to our accumulating material on large landed properties and their management. The subject was long neglected, but in the last two decades information on hacienda history has developed prodigiously. Bazant, a student of economic history and especially the history of Mexican ecclesiastical property, takes almost all the materials for the present study from two private archives, the papers of the hacendado families. The work is a mine of exact information on land values, herds, grain prices, inventories, labor, productivity, and related subjects, with the data increasing in quantity and precision as we approach 1910.

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Ann Arbor

DAVID ROCK. *Politics in Argentina 1890-1930: The Rise and Fall of Radicalism*. (Cambridge Latin American Studies, 19.) New York: Cambridge University Press. 1975. Pp. ix, 315. \$19.50.

Scholars searching for insight into the Argentine crisis long have recognized the critical importance of the fourteen years of political democracy that preceded the 1930 coup, but this period generally has remained an embarrassing lacuna, at least in the English-language historiography. David Rock, a researcher at Cambridge University, attempts to clarify the period by examining the Radical party administrations that held power at the national level during Argentina's brief democratic experiment. The book's analytical framework, based on Argentina's export-dependent economic structure, is sound. The rapid urbanization and massive immigration that accompanied the pre-1914 export boom gave rise to a large and restless urban working class as well as to a frustrated and dependent middle class. Founded in 1891 and led by the enigmatic Hipólito Yrigoyen, the Radical party recruited support from these disaffected sectors, forced the democratization of elections in 1912, and captured the presidency in the 1916 elections.

Rock's analysis of the Radical party's early history is hardly original, for Argentine historians have studied the pre-1916 period intensively. The book's major contribution, which occupies nearly fifty percent of the text, is its detailed analysis of the labor movement and of the Radical government's labor policy during the wave of strikes that accompanied the World War I inflationary spiral. Although Yrigoyen at first cautiously supported

certain sectors of the labor movement, by 1919 he had adopted a policy of open repression. He had little choice, for as Rock demonstrates, the domestic elite-foreign investor alliance mobilized paramilitary groups and was prepared to encourage its contacts in the military to overthrow the government. Yrigoyen, Rock argues, then turned increasingly to personalism and patronage as political appeals. Analysis of the period after 1922 is disappointingly brief and fragmented. The Alvear presidency (1922-28) receives only twenty-three pages of fleeting attention. Although Rock correctly centers his consideration of Yrigoyen's second presidency (1928-30) on the petroleum nationalization issue, he disregards the 1930 congressional crisis and simply ascribes the coup that overthrew Yrigoyen to the Depression's impact. At best, this is a dubious assertion.

Throughout the book, Rock falls into the familiar trap of interpreting Argentine affairs from the perspective of events in the capital city. Politics in the interior receive little attention, while the labor movement outside Buenos Aires suffers the same fate. Rock's total disregard of the huge farm workers' strikes of 1919 and 1920 is a serious omission. Several questionable assumptions also weaken the analysis. For example, Rock's gratuitous use of per capita income comparisons between Argentina and the leading European countries disregards the whole question of price levels and the notoriously high Argentine cost of living. Factual errors, such as the statement that a German philosopher named Peter Krause influenced Yrigoyen's philosophy, may raise a few eyebrows. The numerous incorrect citations, both in the footnotes and the bibliography, will hardly reassure scholars. Despite this volume's significant contribution to Argentine urban labor history, it suffices neither as a history of Argentine politics nor of the Radical party governments during the 1916-30 period.

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MARK FALCOFF and RONALD H. DOLKART, editors.  
*Prologue to Perón: Argentina in Depression and War, 1930-1943*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xviii, 236. \$12.00.

Argentina in the period between the military coup that ousted President Hipólito Yrigoyen in 1930 and the military takeover that launched the political career of Juan D. Perón in 1943 is the subject of this slender volume edited by two historians, Mark Falcoff and Ronald H. Dolkart. To provide a rounded view of that thirteen-year period, the editors, in addition to preparing several chapters themselves, obtained contributions from two other American historians, Arthur Whitaker and Joseph

Tulchin, and from two Argentines, economist Javier Villanueva and film-maker and critic Gustavo Sosa-Pujato.

Collectively, the several writers provide a detailed picture of what has been a controversial but little-studied period. A contemporary nationalist writer once dubbed it the "decade of infamy" and this negative view has influenced much of the writing on the period heretofore. The current volume brings to bear the fruits of recent research as well as a perspective shaped by the passage of time and the realization that despite the advent of Perón the fundamental problems affecting Argentina's stability and self-confidence are still to be resolved.

Following an opening chapter in which Arthur Whitaker presents an overview of the period, the book is organized in topical fashion. Political developments, the economic scene, foreign policy, intellectual currents, popular culture, and the provinces are each treated in a separate chapter. Readers familiar with recent literature will find little that is new in the political sections, which do not seek to do more than synthesize existing knowledge. Tulchin's chapter on foreign policy, however, is a well-documented piece that combines some of his earlier work with later research to explain the roots of Argentine neutrality in World War II. This chapter, together with Falcoff's examination of intellectuals and Sosa-Pujato's treatment of popular culture as reflected in the tango, the myth of Carlos Gardel, and the motion picture, stand out as the most impressive as well as the most valuable.

While the several authors, in keeping with the title of the book, are careful to note those developments which foreshadowed or favored the policies that Perón was later to follow, they resist the temptation to reduce the complexity of the period to a teleological formula. As a result, the period of the thirties and early forties emerges with a richness and variety of its own. Moreover, for the first time since Ysabel Rennie's *The Argentine Republic* (1945) went out of print, university teachers have a highly readable volume to recommend to undergraduates eager to know what Argentina was like before Perón.

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JEAN-PIERRE BLANCPAIN. *Les Allemands au Chili (1816-1945)*. With a foreword by PIERRE CHAUNU. (Latein-amerikanische Forschungen, volume 6.) Cologne: Bohlau Verlag. 1974. Pp. xxxii, 1162, 23 photographs. DM 220.

In 1974 G. F. W. Young's competent work on *Germans in Chile* appeared to set out clearly the nature and volume of nineteenth-century migra-

tion to that distant province. Many readers may believe that one solid book a year on such a modest theme—in fact, only 11,000 Germans went to Chile—would be enough; but now to overwhelm and quickly supersede Young's book is Jean Blancpain's massive 1200-page *histoire totale* on the same subject. It should stand as the last word for some time.

Blancpain has read everything. He works with equal ease and industry in German, French, Spanish, and English. He describes in vivid detail the condition and circumstances of German migration: we are told what the settlers wore, how they farmed, how they endured or perished in the fearful Cape Horn passage, and what they thought of the new land and society around them. More importantly, he has sought to explain the migrants' attitudes as well as the Chileans' reception on their own terms. A simple thing perhaps, but this sort of explanation is only successful when one lives with the sources, reflects deeply on the material, and scrupulously avoids the faintest bit of nationalistic axe-grinding. On all of these counts, Blancpain is impressive.

German immigration to Chile began in 1849. Because there was no free land in the estate-dominated central valley, and because they were unwilling to compete for wages with the impoverished mass of rural workers, the German colonists were led to the sparsely settled country south and beyond the still "unpacified" Araucania—a wet land of lakes and forests. One of the more interesting accomplishments of Blancpain's work is to set the immigrant success alongside an archaic and stagnant Chilean rural society in order to explain the persistent backwardness of the Spanish American countryside. Like such other French scholars of Latin America as François Chevalier and Jean-Pierre Berthe, Blancpain apparently spent years in the country as a secondary school teacher. The enormous merit of such an experience (perhaps it could be compared favorably with the American fellowship programs) is shown by his broad and profound understanding of Chile. Indeed, the story of German immigration is surrounded by such an informed discussion of Chilean society and politics that the book may be read

with profit by Latin Americanists uninterested in the theme of migration itself.

For North American scholars, Blancpain's book has value for the comparative study of migration. Some 5,500,000 Germans moved overseas during the nineteenth century: a few to southern Brazil and Argentina, some to Chile, but most, of course, to the United States. How can the differential rates of acculturation be explained? Why did immigrants fiercely retain their cultural identity in one region and rush to assimilate in another? This is a central theme of Blancpain's work. In his explanation, education and voluntary association are important. Because the Chilean Republic was unable and often unwilling to assume responsibility following the decline of the colonial Church, the immigrants were required to provide their own schools; because there was little of the North American pressure for cultural conformity, the Germans—many were liberal and Protestant—set off by their own industry and sacrifice, joined together in innumerable *Vereine*. The initial cultural coherence of the colonists was reinforced in the 1880s when the Chilean elite invited German schoolmasters and army officers to help expand the system of public education and to professionalize the military. This fascination of "bewitchment" [*l'ensorcellement allemand*] by German technical skill and values, although shaken by the Great War, lasted to 1939. Thus, as Young points out, in Chile, alone among American Republics, "one's entire primary and secondary education may still be legally obtained in German schools where even the language of instruction is German," and the influence of the immigrant group is out of all proportion to its size.

And so it is with the book itself. Blancpain is concerned with a tiny number of European migrants in a distant corner of the world. He has dealt with this subject definitively and even excessively; but the way he has posed his questions and invested them with meaning projects the importance of his work far beyond the boundaries of Chile.

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University of California,  
Davis

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

While most historians are agreed, I think, that we should give serious attention to techniques and symbol-systems developed by the natural sciences and adapted by the so-called social sciences, most of us also admit that we must guard against mindless adaptations of such techniques and symbols that become merely faddish. Paula Sutter Fichtner's "Dynastic Marriage in Sixteenth-Century Habsburg Diplomacy and Statecraft: an Interdisciplinary Approach" (*AHR*, April 1976) strikes me as just the sort of thing we must guard against. She has done fine research and compiled a worthy study, then, it would seem, has larded it with quotations from Claude Lévi-Strauss, references to the Ramah Navaho, and tables of symbols such as *a b c*, *X*, and  $\infty$ . She might better have stuck with Frederick III's A.E.I.O.U.

To justify her approach Fichtner sets up her straw men—most Western historians since the eighteenth century, who "dismiss the court and family practices of European royalty as unworthy of study." Either I know the wrong people or what she says, at least in regard to scholars of Early Modern Europe, is ridiculous. In my *Philip II of Spain* (London, 1975) I pay close attention to the

intricacies of Habsburg marriage diplomacy, but never thought that I was breaking new ground or needed the authority of Lévi-Strauss to protect me against colleagues who might be "mystified" by marriage diplomacy.

Of the modern works Paula Fichtner musters in support of her contention, Gaston Zeller's volume in Pierre Renouvin's *Histoire des relations internationales* seems to me largely a survey, and Garrett Mattingly's *Renaissance Diplomacy* primarily a study of the development of the institution of resident embassies. She should have consulted Mattingly's *Catherine of Aragon*, which is nothing if not a brilliant study of marriage diplomacy and how it really worked. Fichtner does not rest with us moderns, but also points out that Voltaire, Erasmus, and Thomas More (in *Utopia*) found marriage diplomacy unimportant and useless. I suggest that she stick with her statesmen and their writings and conduct and pay no more heed than they did to the literati of their age in this matter.

I find her detailed description of Ferdinand I's dynastic diplomacy most enlightening, as well as her discussion of the marriage patterns of the Austrian nobility and the other German dynasts. My own researches into the family ties of the Castilian nobility reveal similar incidences and a fascinating pattern of territorial concerns that are similar, if on a smaller scale, to those of the greater European dynasts. Similar patterns of connections and interests appear in the Low Countries, northern France, Burgundy, the Franche Comté, and Lorraine, and in the Italian states. One really does not need the Ramah Navaho to find others doing what the Habsburgs did.

With the assistance of a good map, I find the explanations offered by the sixteenth-century Habsburgs for their marriage policies perfectly satisfying. In no way can I see that it is of methodological, let alone historical, usefulness to know that "in view of Lévi-Strauss's remarks about peoples' lack of awareness of the underlying structure of their customs [and who needs him to tell us this?], it is not surprising that Ferdinand was

oblivious to the levels upon which this exchange of gifts [in line with the marriage contracts] took place." So what? In the context of his own times he was aware of what he wanted and how to get it.

To look then at dynastic diplomacy through the eyes of anthropologists and sociologists, as Fichtner wants us to do, is at best mildly interesting, but hardly consequential or necessary to our understanding of it. If Lévi-Strauss, working today, seems wiser than Voltaire, so does Giambattista Vico, Voltaire's slightly older and then little-known contemporary. I suspect that today Vico is held in higher regard as a contributor to the historical sciences than Voltaire; and I doubt that most of us are as ignorant of the antiquity, extent, and strength of family bonds at any level of human society as Fichtner seems to believe.

Her article reflects, I fear, an unfortunate state of affairs that cannot be briefly described and in which the problems are many and complex. To put it roughly, I find that too many historians are witlessly jumping on the bandwagon of the social sciences, which have more and more become ahistorical, and whose vogue among undergraduates seems almost tragic. Common sense (as generally understood) is ignored; standard English gives way to jargon, and historical sensitivity gets lost in the shuffle. In the United States today history, which should command and marshal the social sciences, risks becoming their echo; they provide us with models and we apply them—and get published in the *AHR*.

More useful to the profession than the application of Fichtner's techniques to the sixteenth-century Habsburgs would be their application to the *AHR* and its Board of Editors. The tribal connections seem obvious for the April issue in three of four cases. In hers we find a member of the Board from the same tribe (Brooklyn College); in another, we find a student of a colleague of a Board Member, all of the Columbia University tribe; and in a third, a student of the western pole of the Russian Studies axis, the eastern pole of which sits on the Board. I have not sufficient data to explain the fourth; nor sufficient time to undertake the study of such connections through the years. It might be recommended to a graduate student in anthropology as a good dissertation topic.

PETER O'M. PIERSON  
*University of Santa Clara*

#### TO THE EDITOR:

Peter Pierson's comments fall into several categories—historiography, pedagogy, and questions of professional ethics. I shall try to deal with the major points raised in all three areas.

As far as historiographical considerations go, he and I seem to differ markedly in our construction of the phrase "marriage diplomacy and how it really worked." Indeed, it was just this concern that led me to introduce anthropological models into my study. Since Pierson's book on Philip II had not come out at the time I finished the last draft of my article (September 1974), I cannot comment on his treatment of "the intricacies of Habsburg marriage diplomacy." Since he points to the late Garrett Mattingly's *Catherine of Aragon* as a good explanation of the practice, however, it might be well to turn to that text for a moment and see what it does tell us about dynastic marriage. Mattingly's first chapters are eloquent and at times moving cameos of the marital fate of Ferdinand and Isabella's children. But he seems to explain these marriages (for example, pp. 13, 17) simply as efforts to secure Spain's military and political position. I do not deny in my article that this motive played a role in dynastic marriage. But in my discussion of the exchange system, I hope to have shown that its function was far more complex than even a master craftsman such as Mattingly perceived. As a result, he has left us with an explanation that is only partial at best. One's attitude toward all of this depends, I think, on the level of explanation with which one is satisfied. While no mortal historian can be equally competent in all academic disciplines, there are times when one's choice of subject forces one beyond one's training in order to make more adequate statements about the material under study. Explanations of dynastic marriage constitute one of those moments. As to the brief interjection of the Ramah Navaho into my work, I have never thought a historian exceeds his bounds when he relates the experience of his subject to that of humankind in general.

In his distress over the negative impact of the social sciences on historical studies in our colleges, Peter Pierson joins a growing and ever more public chorus, as shown by the recent appearance of an article by Arthur M. Schlesinger, jr. expressing similar views ("Do We Care About Our History?", *Wall Street Journal*, July 1, 1976, 12). I share this concern. I also think, however, that in the zeal with which some of us defend our beleaguered field, we overlook the fact that many students flock to subjects such as psychology and sociology not only because of the largely presentist focus of the latter, though some of us might like to think so. Rather, many of our undergraduates believe deeply that these disciplines are giving them something that history does not. Questioning a class on this point once, I was told that they all took sociology because "it tells us something about people." I am convinced that history has something to tell



students "about people" too, but I am equally persuaded that we shall not be able to do this unless we show them that we can learn from the past as well as the present by some of the techniques and analytic categories that engage them so much. I agree with Pierson that many social scientists are discouragingly ahistorical, but this is frequently not a *déformation professionnelle* as much as it is the result of their ideological orientation in which contemporary concerns bulk large. This does not mean that all historical materials are appropriate to the type of dissection to which I subjected Ferdinand's marriage policies. When dealing with a theme like dynastic marriage, however, which had a large role in premodern society and which *can* be more fully understood through application of behavioral models, historians should not hesitate to use them. After all, our primary task is to recover the past as accurately as possible, a mission that brings with it considerable methodological flexibility.

The final part of Peter Pierson's letter implies that some connection exists between the publication of my article and the presence of my colleague, Elizabeth A. R. Brown, on the editorial board of the *AHR*. This is utter nonsense. I first submitted the essay to the *AHR* in January of 1973; it was returned to me on June 20 of that year by Robert K. Webb with readers' comments calling for certain revisions. It was resubmitted August 28, 1974. The letter of acceptance was dated April 11, 1975. Elizabeth Brown tells me that the matter of her appointment to the Board of Editors was first broached during the AHA convention in December 1975; the letter of appointment is dated February 13, 1976. It appears that Pierson is, after all, more of a structuralist than he lets on. I find a pattern of exchange in marriage arrangements; he sees patterns of conspiracy in the acceptance of articles by the *AHR*. Far be it from me to discourage this approach; I would only recommend more careful research before jumping to conclusions.

PAULA SUTTER FICHTNER  
Brooklyn College,  
City University of New York

TO THE EDITOR:

I was, needless to say, delighted with Jerah Johnson's enthusiastic review of my book, *Style in History* (*AHR*, April 1976, 352.) My pleasure in his favorable appraisal was somewhat compromised, though, by an important typographical error or slip of the pen that was allowed to find its way into print. Johnson says that I come down "on the side of the idealists with a splendidly and sensibly

worked argument. . . ." Actually, of course, my entire book is an extended argument in behalf of critical Realism, not Idealism: while, I argue, style discloses unique perspectives on the past, that past is objectively there, accessible matter for scientific inquiry.

PETER GAY  
Yale University

TO THE EDITOR:

My mistake. My apologies.

JERAH JOHNSON  
The University of New Orleans

TO THE EDITOR:

In the April issue of the *AHR* I reviewed Jay W. Baird's book on Nazi war propaganda. There was a significant misprint at the beginning of the review, an error not of my doing. Where the journal printed "In the beginning was the world" it should have read "In the beginning was the word" (*Am Anfang war das Wort*). I would appreciate it if you would note this correction.

ROBERT HERZSTEIN  
University of South Carolina

TO THE EDITOR:

I have sent the attached letter to Bruce Webster: Dear Mr. Webster:

An apology is owed to you for an egregious error in the text of my review of your book, *Scotland from the Eleventh Century to 1603*, in the April 1976 issue of the *American Historical Review*. The text as printed reads, "There are chapters, neither numerous nor reliable, on the narrative sources. . . ." What I wrote was, "There are chapters on the narrative sources (neither numerous nor reliable). . . ." My intent, of course, was to describe the sources, not your chapters. How this transposition got made I don't know. The *Review* normally sends a copy-edited text of a review to its author before printing it; I assume—though I have no specific memory of receiving one—that this was done, and that I simply did not notice that this change had been made, though quite honestly I find it hard to believe that I could have been so careless. I am profoundly sorry for this regrettable and embarrassing blunder; as the rest of my review makes clear, I think very highly of your excellent and very useful book. I am sending a copy of this letter to the editor of the *Review* with a request that it be published as soon as possible.

MAURICE LEE, JR.  
Douglass College

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## Recent Deaths

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DWIGHT LOWELL DUMOND passed away on May 30, 1976, after a short illness. Born in Kingston, Ohio, on August 27, 1895, Dumond received his B.A. from Baldwin Wallace College in 1920, his M.A. from Washington University in 1928, and his Ph.D. from the University of Michigan in 1930. He later received honorary degrees from Baldwin Wallace and Northern Michigan University.

After teaching for a year at Ohio Wesleyan University, Dumond joined the University of Michigan faculty in 1930, and he remained at Michigan until his retirement in 1965. In 1939 he was the Commonwealth Lecturer at University College, University of London, and after his retirement he taught at Howard University and Colgate.

A stimulating teacher of both undergraduate and graduate students and the director of a substantial number of doctoral dissertations, Dumond became well known on the Michigan campus for his elegantly phrased, beautifully delivered, and sometimes emotion-filled lectures. In addition to his courses in the middle-period of American history, he pioneered in the teaching of twentieth-century American history, and he wrote the first important textbook in the field, *Roosevelt to Roosevelt* (New York, 1937). He took an active role in the affairs of the university, which honored him in 1963 with its Distinguished Faculty Achievement Award.

With a lively interest in the historical profession, Dumond served as a member and as chairman of the executive committee of the Mississippi Valley Historical Association (now the Organization of American Historians) and as the organization's president in 1948-1949. In 1943 he was a member of a joint committee of the American Historical Association and the National Council for the Social Studies that examined the teaching of history in the nation's schools and colleges. He also served on the board of editors of the *Journal of Southern History*.

Dumond first made his mark as a historian of the Civil War era with the publication of *The Secession Movement* (New York, 1931). He supplemented this important monograph with *Southern Editorials on Secession* (New York, 1931). It was the first volume published by the American Historical Association on the Albert J. Beveridge Fund.

Dumond became best known as a historian of the antislavery movement. He discovered and edited a substantial amount of new material in this field, notably the letters of Theodore Dwight Weld, Angelina Grimké Weld, Sarah Grimké, and James Gillespie Birney, and he presented his interpretation of the movement in *Antislavery Origins of the Civil War in the United States* (Ann Arbor, 1939) and the monumental *Antislavery: The Crusade for Freedom in America* (Ann Arbor, 1961). The latter work, winner of the Anisfield-Wolf Award, was accompanied by *A Bibliography of Antislavery in America* (Ann Arbor, 1961). Along with Gilbert Barnes, Dumond challenged the view that the antislavery movement centered in New England and contended that a band of Middle Westerners were primarily responsible for converting the North to the antislavery cause. Dumond's antislavery publications were cited in oral argument by counsel for the plaintiffs in the school segregation cases to supplement their written briefs.

Dwight Lowell Dumond is survived by his wife, the former Irene M. Hettel, two children, two grandchildren, one great grandchild, and three sisters. He will be remembered with affection by those who knew him for the warmth of his personality, his courtly manners, his loyalty to his friends, his love of the outdoors, his considerable talents as a cook, and the vigor with which he expressed his opinions.

SIDNEY FINE  
*University of Michigan*

## Other Books Received

Books listed were received by the *AHR* between July 1 and September 1, 1976. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

- ASHWORTH, WILLIAM. *A Short History of the International Economy since 1850*. Reprint. London: Longman. 1976. Pp. viii, 318. \$15.00.
- BERNSTEIN, RICHARD J. *The Restructuring of Social and Political Theory*. New York: Harcourt Brace Jovanovich. 1976. Pp. xxiv, 286. \$17.95.
- BERRY, BOYD M. *Process of Speech: Puritan Religious Writing and Paradise Lost*. Baltimore: Johns Hopkins University Press. 1976. Pp. xi, 305. \$13.50.
- BROMLEY, J. S., and KOSSMANN, E. H., editors. *Britain and the Netherlands*. Volume 5, *Some Political Mythologies. Papers Delivered to the Fifth Anglo-Dutch Historical Conference*. The Hague: Martinus Nijhoff. 1975. Pp. viii, 212. fl 65.
- CHACE, JAMES, and RAVENAL, EARL C., editors. *Atlantis Lost: U.S.-European Relations after the Cold War*. (A Council on Foreign Relations Book.) New York: New York University Press. 1976. Pp. xxviii, 273. \$15.00.
- CHOMSKY, NOAM, et al. *La crise de l'impérialisme et la troisième guerre mondiale*. Paris: François Maspero. 1976. Pp. 278.
- CLAUDE, RICHARD P., editor. *Comparative Human Rights*. Baltimore: Johns Hopkins University Press. 1976. Pp. xvi, 410. \$16.50.
- COHANE, JOHN PHILIP. *The Key*. With a preface by CYRUS H. GORDON. New York: Schocken Books. 1976. Pp. 288. \$5.95.
- COING, HELMUT, editor. *Handbuch der Quellen und Literatur der neueren europäischen Privatrechtsgeschichte*. Volume 2, *Neuere Zeit (1500-1800): Das Zeitalter des gemeinen Rechts; part 2, Gesetzgebung und Rechtsprechung*. Munich: C. H. Beck'sche Verlagsbuchhandlung. 1976. Pp. xxix, 1445. DM 195.
- COOMBS, CHARLES A. *The Arena of International Finance*. New York: John Wiley and Sons. 1976. Pp. xvii, 243. \$12.95.
- CURTISS, VIENNA IONE. *A Visual History of Western Culture: Pageant of Art*. Baltimore: Collector's Choice, French/Bray. 1976. Pp. 531. \$30.00.
- DE SÁ, A. MOREIRA, et al., editors. *Bibliographie internationale de l'histoire des universités*. Volume 2, *Portugal, Leiden, Pécs, Franeker, Basel*. (Commission internationale pour l'histoire des universités, études et travaux, number 5.) Genève: Librairie Droz. 1976. Pp. x, 219.
- DESLANDRES, YVONNE. *Le costume, image de l'homme*. (L'aventure humaine.) Paris: Albin Michel. 1976. Pp. 298.
- EISENSTADT, S. N., and CURELARU, M. *The Form of Sociology—Paradigms and Crises*. New York: John Wiley and Sons. 1976. Pp. xvii, 386. \$17.95.
- FARRER, CLAIRE R., editor. *Women and Folklore*. Austin: University of Texas Press. 1976. Pp. xvii, 99. Cloth \$10.00, paper \$3.45.
- FEBVRE, LUCIEN, and MARTIN, HENRI-JEAN. *The Coming of the Book: The Impact of Printing, 1450-1800*. Edited by GEOFFREY NOWELL-SMITH and DAVID WOOTTON. Translated by DAVID GERARD. Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 378. \$27.00.
- GAY, PETER, editor. *Eighteenth-Century Studies Presented to Arthur M. Wilson*. Reprint. New York: Russell and Russell. 1975. Pp. viii, 197. \$13.00.
- GILLISPIE, CHARLES COULSTON, et al., editors. *Dictionary of Scientific Biography*. Volume 14, *Addison Emery Verrill-Johann Zwielfer*. New York: Charles Scribner's Sons. 1976. Pp. 640. \$40.00.
- GRUNEBaum, G. E. VON. *Muhammadian Festivals*. Introduction by C. E. BOSWORTH. Reprint. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. viii, 107. \$7.00.
- GUILLAUME, P., et al. *Nouvelle histoire économique*. Volume 2, *Le XX<sup>e</sup> siècle: Les incertitudes du premier XX<sup>e</sup> siècle*. Paris: Armand Colin. 1976. 68 fr.
- HIERRMANN, JOACHIM, and SELLENOW, IRMGARD, editors. *Die Rolle der Volksmassen in der Geschichte der vorkapitalistischen Gesellschaftsformationen: Zum XIV. Internationalen Historiker-Kongress in San Francisco 1975*. (Veröffentlichungen des Zentralinstituts für Alte Geschichte und Archäologie der Akademie der Wissenschaften der DDR, volume 7.) Berlin: Akademie-Verlag. 1975. Pp. 322. M 28.
- HERZ, JOHN H. *The Nation-State and the Crisis of World Politics: Essays on International Politics in the Twentieth Century*. New York: David McKay. 1976. Pp. viii, 307. \$4.95.
- HOWARD, MICHAEL. *War in European History*. New York: Oxford University Press. 1976. Pp. x, 165. \$8.00.
- JANKO, JAN. *Vznik a rozklad mechanistické koncepce ve fyziologii: Osudy mechanismu v zrcadle syntetických zpracování fyziologie v 2. polovině 19. a na začátku 20. století* [The Rise and Fall of Mechanistic Concepts in Physiology: The Fate of Mechanism in the Light of Synthetic Theories in Physiology in the Latter Half of the Nineteenth Century and the Beginning of the Twentieth]. (Práce z dějin přírodních věd, number 5.) Prague: Ústav československých a světových dějin ČSAV. 1975. Pp. 382.
- KATZNELSON, IRA. *Black Men, White Cities: Race, Politics, and Migration in the United States, 1900-30, and Britain, 1948-68*. Reprint. Chicago: University of Chicago Press. 1976. Pp. xxiv, 219. \$3.95.
- KLEIN, FRITZ, and DRECHSLER, KARL, et al., editors. *Die USA und Europa 1917-1945: Studien zur Geschichte der Beziehungen zwischen den USA und Europa von der Grossen Sozialistischen Oktoberrevolution bis zum Ende des zweiten Weltkrieges*. (Schriften des Zentralinstituts für Geschichte, volume 45.) Berlin: Akademie Verlag. 1975. Pp. 301.
- LINDBERG, LEON N. *Politics and the Future of Industrial Society*.

- (Comparative Studies of Political Life.) New York: David McKay. 1976. Pp. xii, 286. Cloth \$12.50, paper \$6.95.
- LOEBL, EUGEN. *My Mind on Trial*. New York: Harcourt Brace Jovanovich. 1976. Pp. 235. \$8.95.
- MCCARTHY, JOHN F. *The Science of Historical Theology*. Volume 1, *Elements of a Definition*. Rome: Propaganda Mariana. 1976. Pp. xii, 195. \$6.00.
- MCCLELLAND, DAVID C. *The Achieving Society*. Reprint. New York: John Wiley and Sons. 1976. Pp. xv, 512. \$17.95.
- MCNEILL, JOHN J. *The Church and the Homosexual*. Kansas City: Sheed Andrews and McMeel. 1976. Pp. xiii, 211. \$10.00.
- MANDEL, ERNEST. *Late Capitalism*. Translated by JORIS DE BRES. London: Humanities Press. 1975. \$25.00.
- MAY, ANTOINETTE. *Different Drummers: They Did What They Wanted*. Millbrae, Cal.: Les Femmes. 1976. Pp. iii, 156. \$4.95.
- MEYERS, JEFFREY. *A Fever at the Core: The Idealist in Politics*. New York: Barnes and Noble. 1976. Pp. 172. \$15.00.
- MIRSKY, JEANNETTE. *Houses of God*. Reprint. Chicago: University of Chicago Press. 1976. Pp. 235. \$7.95.
- OLSON, MCKINLEY C. *Unacceptable Risk: The Nuclear Power Controversy*. New York: Bantam Books. 1976. Pp. 309. \$2.25.
- OWEN, A. L. RIESCH. *Selig Perlman's Lectures on Capitalism and Socialism*. Madison: University of Wisconsin Press. 1976. Pp. xviii, 183.
- PALMER, CHRISTOPHER. *Delius: Portrait of a Cosmopolitan*. Foreword by ERIC FENBY. New York: Holmes and Meier. 1976. Pp. xi, 199. \$19.50.
- PREZZOLINI, GIUSEPPE. *The Case of the Casa Italiana*. New York: American Institute of Italian Studies. 1976. Pp. xi, 63.
- RAYNOR, HENRY. *Music and Society since 1815*. New York: Schocken Books. 1976. Pp. viii, 213. \$15.00.
- ROBERTS, JOAN L. editor. *Beyond Intellectual Sexism: A New Woman, A New Reality*. New York: David McKay. 1976. Pp. xiv, 386. Cloth \$14.95, paper \$7.95.
- RUDENKO, G. F., et al., editors. *The Revolutionary Movement of Our Time and Nationalism*. Translated by VIC SCHNEIERSON. Moscow: Progress Publishers; distributed by Imported Publications, Chicago. 1975. Pp. 284. \$3.25.
- SCHWARTZ, TED. *Coins as Living History*. New York: Arco Publishing. 1976. Pp. 224. \$8.95.
- STOLZ, BENJAMIN A., and SHANNON, RICHARD S., editors. *Oral Literature and the Formula*. Ann Arbor: Center for the Coordination of Ancient and Modern Studies, University of Michigan. 1976. Pp. xvii, 290. \$4.00.
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- HIRSCH, ANDREW VON. *Doing Justice: The Choice of Punishments*. With an introduction by WILLARD GAYLIN and DAVID J. ROTHMAN. New York: Hill and Wang. 1976. Pp. xli, 179. \$4.95.
- ZAEHNER, R. C. *The Teachings of the Magi: A Compendium of Zoroastrian Beliefs*. Reprint. New York: Oxford University Press. 1976. Pp. 156. \$2.50.
- maps by R. NEVILLE HADCOCK. 2d edition. New York: Longman. 1976. Pp. xxviii, 246. \$35.00.
- DE MÉZIERES, PHILIPPE. *Letter to King Richard II: A Plea Made in 1395 for Peace Between England and France*. Introduced and translated by G. W. COOPLAND. New York: Barnes and Noble. 1976. Pp. xxxiv, 152. \$30.00.
- DE ROOVER, RAYMOND. *Business, Banking, and Economic Thought: In Late Medieval and Early Modern Europe*. Edited by JULIUS KIRSHNER. Chicago: University of Chicago Press. 1974. Pp. viii, 383. Cloth \$15.00, paper \$4.95.
- LANDER, J. R. *Crown and Nobility, 1450-1509*. Montreal: McGill-Queen's University Press. 1976. Pp. x, 340. \$23.50.
- MCGARRY, DANIEL D. *Medieval History and Civilization*. New York: Macmillan Company. 1976. Pp. xv, 742. \$13.95.
- MYNORS, R. A. B., and THOMSON, D. F. S., translators. *The Correspondence of Erasmus*. Volume 3. *Letters 298 to 445 (1514 to 1516)*. Buffalo: University of Toronto Press. 1976. Pp. xvi, 392. \$25.00.
- SEAHOIM, CHARLES H. *The Kelts and the Vikings*. New York: Philosophical Library. 1974. Pp. 448. \$10.00.
- WEINBAUM, MARTIN, editor. *The London Eyre of 1276*. (London Record Society Publications, volume 12 for 1976.) London: London Record Society. 1976. Pp. xli, 189.
- WOLFF, ROBERT LEE. *Studies in the Latin Empire of Constantinople*. London: Variorum Reprints. 1976. Various pages. \$37.50.

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- ADDY, JOHN. *The Textile Revolution*. (Seminar Studies in History.) 1976. New York: Longman. Pp. vi, 122. \$2.75.
- AIRS, MALCOLM. *The Making of the English Country House, 1500-1640*. Reprint. New York: Universe Books. 1976. Pp. viii, 208. \$17.50.
- BEAULIEU, ANDRÉ, and HAMELIN, JEAN. *La presse québécoise des origines à nos jours*. Volume 2, 1800-1879. Quebec: Les Presses de l'Université Laval. 1975. Pp. xv, 350. \$11.95.
- CARABELLI, GIANCARLO. *Tolandiana: Materiali bibliografici per lo studio dell'opera e della fortuna de John Toland (1670-1722)*. (Centro di studi del pensiero filosofico del cinquecento e del seicento in relazione ai problemi della scienza. Series II, Strumenti bibliografici, number 3. Florence: La Nuova Italia Editrice. 1975. Pp. xii, 407. L. 8000.
- GASH, NORMAN. *Peel*. New York: Longman. 1976. Pp. xiii, 319. \$17.50.
- GREGG, PAULINE. *Black Death to Industrial Revolution: A Social and Economic History of England*. New York: Barnes and Noble. 1976. Pp. 344. \$12.50.
- GWYN, JULIAN, editor. *The Royal Navy and North America: The Warren Papers, 1736-1752*. (Publications of Navy Records Society, volume 118.) London: Navy Record Society. 1975. Pp. xlv, 465. L. 6.00.
- HALL, A. RUPERT, and TILLING, LAURA, editors. *The Correspondence of Isaac Newton*. Volume 6, 1713-1718. Published for the Royal Society. New York: Cambridge University Press. 1976. Pp. xxxviii, 499. \$65.00.
- KINVIG, R. H. *The Isle of Man: A Social, Cultural, and Political History*. Rutland, Vt.: Charles E. Tuttle. 1975. Pp. xv, 198. \$14.95.
- MUNRO, JOHN A., and INGLIS, ALEX I., editors. *Mike: The Memoirs of the Right Honourable Lester B. Pearson*. Volume 3, 1957-1968. New York: Quadrangle Books. 1976. Pp. xii, 338. \$12.50.
- PALMER, KINGSLEY. *The Folklore of Somerset*. (The Folklore of the British Isles.) Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 186. \$14.50.
- PALMER, ROY. *The Folklore of Warwickshire*. (The Folklore of the British Isles.) Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 208. \$11.50.
- PRICE, MARY R., and MATHER, C. E. L. *A Portrait of Britain*

## ANCIENT

- GREENLAW, JEAN-PIERRE. *The Coral Buildings of Suakin*. Boston: Oriel Press. 1976. Pp. 132. \$24.00.
- SALONEN, ARMAS. *Jagd und Jagdtiere im Alten Mesopotamien*. Helsinki: Suomalainen Tiedeakatemia. 1976. 313.

## MEDIEVAL

- COWAN, IAN B., and EASSON, DAVID E. *Medieval Religious Houses: Scotland*. Foreword by DAVID KNOWLES and

- under Tudors and Stuarts, 1485-1688. (The Oxford Introduction to British History.) Reprint. New York: Oxford University Press. 1976. Pp. 256.
- ROSS, ANNE. *The Folklore of the Scottish Highlands*. (The Folklore of the British Isles.) Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 174. \$9.50.
- ROWLING, MARJORIE. *The Folklore of the Lake District*. (The Folklore of the British Isles.) Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 184. \$9.50.
- SIMPSON, JACQUELINE. *The Folklore of the Welsh Border*. (The Folklore of the British Isles.) Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 210. \$11.50.
- THIRSK, JOAN. *The Restoration*. (Problems and Perspectives in History.) New York: Longman. 1976. Pp. xxiv, 205. \$4.00.

## FRANCE

- BARBER, NOEL. *The Week France Fell*. New York: Stein and Day. 1976. Pp. 321. \$10.95.
- BOULIND, RICHARD. *Cambacères and the Bonapartes: Unpublished Papers of Jean-Jacques-Régis Cambacères, Second Consul and later Arch-Chancellor, relating to the Emperor Napoleon and his Circle; A Calendar*. Section 1, *Cambacères: Letters to Napoleon, 1805-1814*; section 2, *Papers on the Personal and Dynastic Interests of Napoleon*. New York: H. P. Kraus. 1976. Pp. 157. \$9.00.
- CORVISIER, ANDRÉ. *Armées et sociétés en Europe de 1494 à 1789*. (L'historien, number 27.) Paris: Presses universitaires de France. 1976. Pp. 222.
- FOISIL, MADELEINE, edited with an introduction by. *Mémoires du Président Alexandre Bigot de Monville: Le Parlement de Rouen, 1640-1643*. (Publications de la Sorbonne, Recherches, number 28. Travaux du Centre de recherches sur la civilisation de l'Europe moderne, volume 19.) Paris: Editions A. Pedone. 1976. Pp. 301.
- FORSTER, ROBERT, AND RANUM, OREST, editors. *Family and Society: Selections from the Annales—Economies, Sociétés, Civilisations*. Translated by ELBORG FORSTER and PATRICIA M. RANUM. Baltimore: Johns Hopkins University Press. Pp. x, 261. \$3.45.
- HARTMANN, PETER CLAUS. *Pariser Archive, Bibliotheken und Dokumentationszentren: zur Geschichte des 19. und 20. Jahrhunderts. Eine Einführung in Benützungspraxis und Bestände für Historiker, Politologen und Journalisten*. (Dokumentation Westeuropa, number 1.) Munich: Verlag Dokumentation. 1976. Pp. 131. DM 28.
- MINTON, ROBERT. *John Law: The Father of Paper Money*. New York: Association Press. 1975. Pp. 288. \$10.00.
- NÔEL, JEAN-FRANÇOIS. *Le Saint-Empire*. (Que sais-je?) Paris: Presses Universitaires de France. 1976. Pp. 126.
- RICE, HOWARD C., JR. *Thomas Jefferson's Paris*. Princeton: Princeton University Press. 1976. Pp. ix, 156. Cloth \$18.50, paper \$8.75.
- VANDENBOSSCHE, ANDRÉ. *Contribution à l'histoire des sources du droit commercial: Un commentaire manuscrit de l'ordonnance de mare 1673*. Paris: Éditions Cujas. 1976. Pp. 125.
- WALSH, JAMES E., editor. *Mazarinades: A Catalogue of the Collection of Century French Civil War Tracts in the Houghton Library, Harvard University*. (Houghton Library Bibliographical Contributions.) Boston: G. K. Hall. 1976. Pp. xi, 459.

## SPAIN AND PORTUGAL

- CUENCA TORIBIO, JOSÉ MANUEL. *Del antiguo al nuevo régimen*. (Historia de Sevilla, number 5.) Sevilla: Publicaciones de la Universidad de Sevilla. 1976. Pp. 146. 100 ptas.
- DOMÍNGUEZ ORTIZ, ANTONIO, and AGUILAR PIÑAK, FRANCISCO. *El barroco y la ilustración*. (Historia de Sevilla, number 4.) Sevilla: Publicaciones de la Universidad de Sevilla. 1976. Pp. 290. 175 ptas.
- VIDAL I BARRAQUER, ARXIU. *Església i estat durant la segona*

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Compiled by ANN HIGGINBOTHAM

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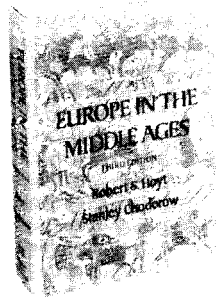
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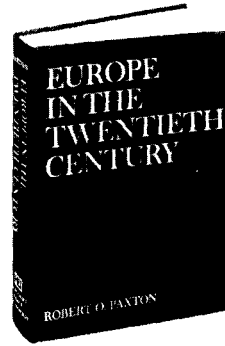
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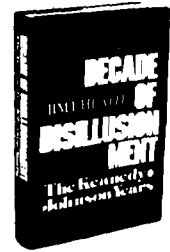
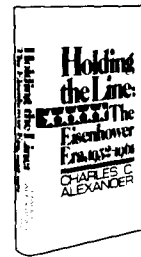
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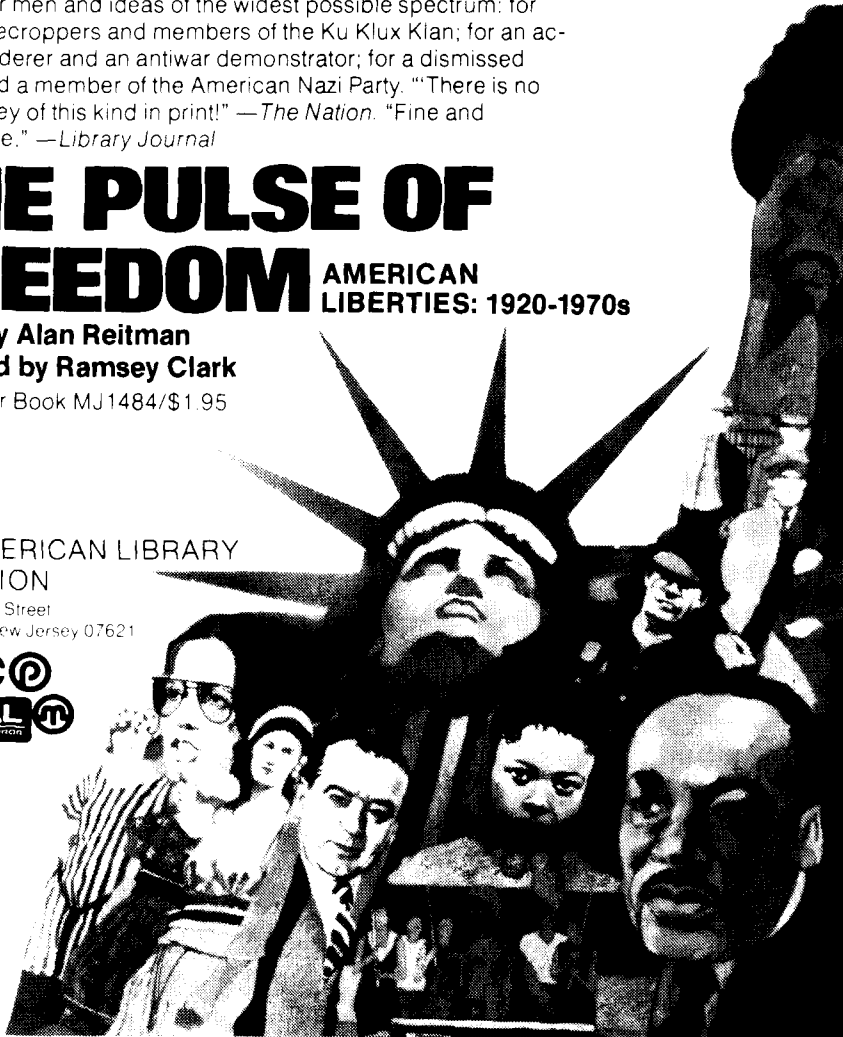
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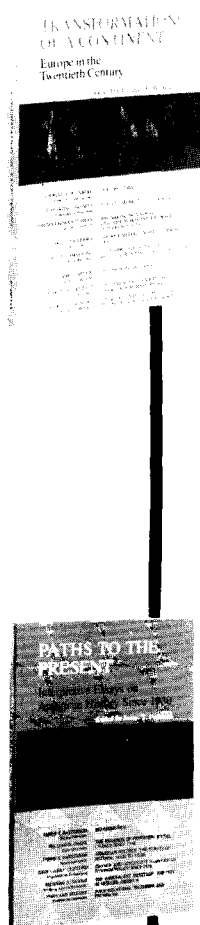
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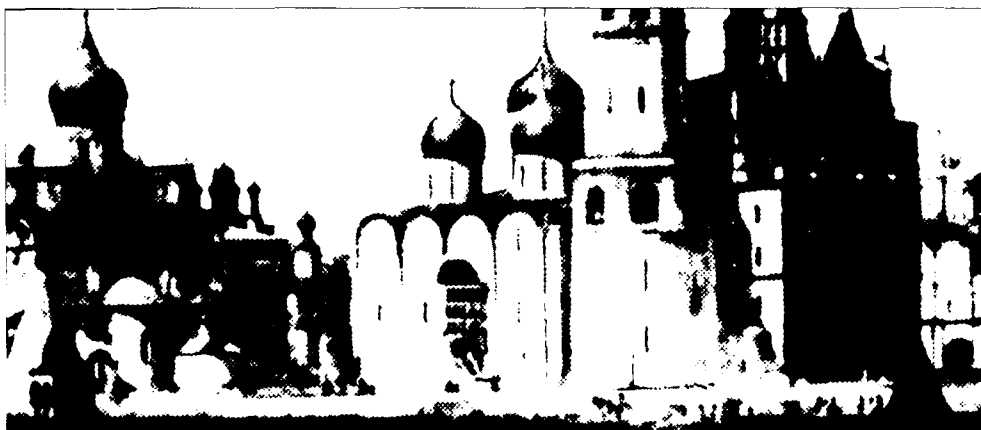
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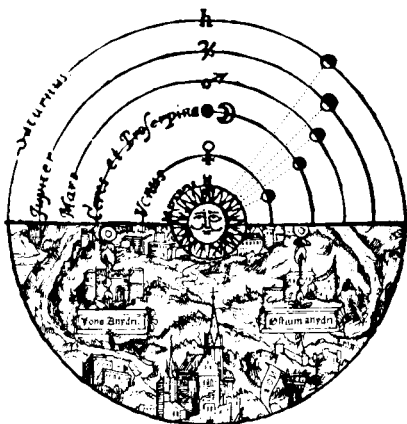
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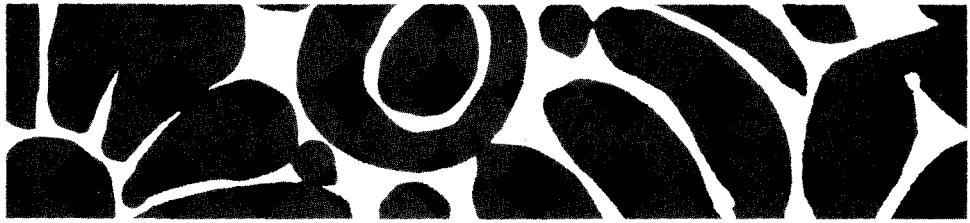
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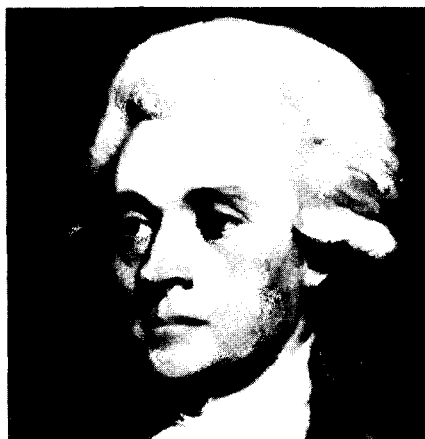
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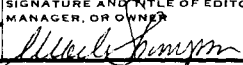
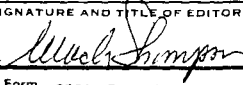
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